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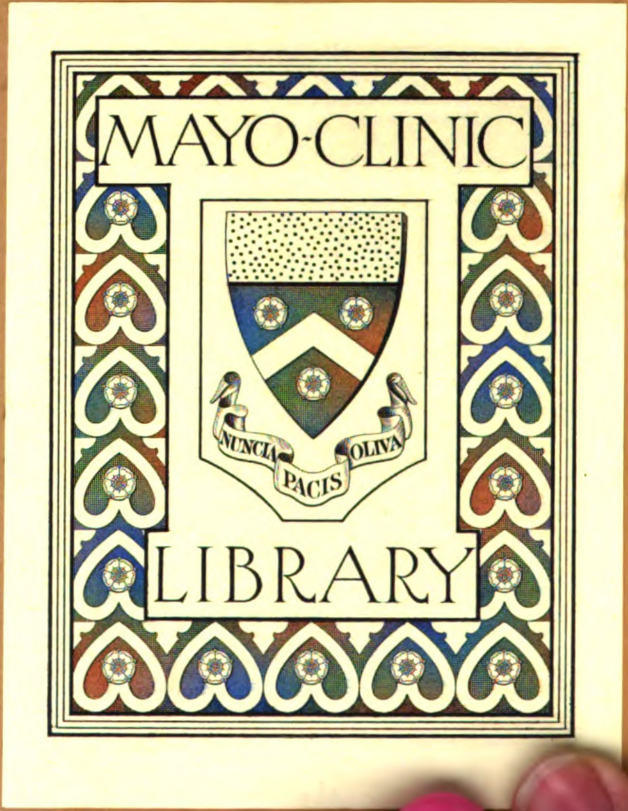
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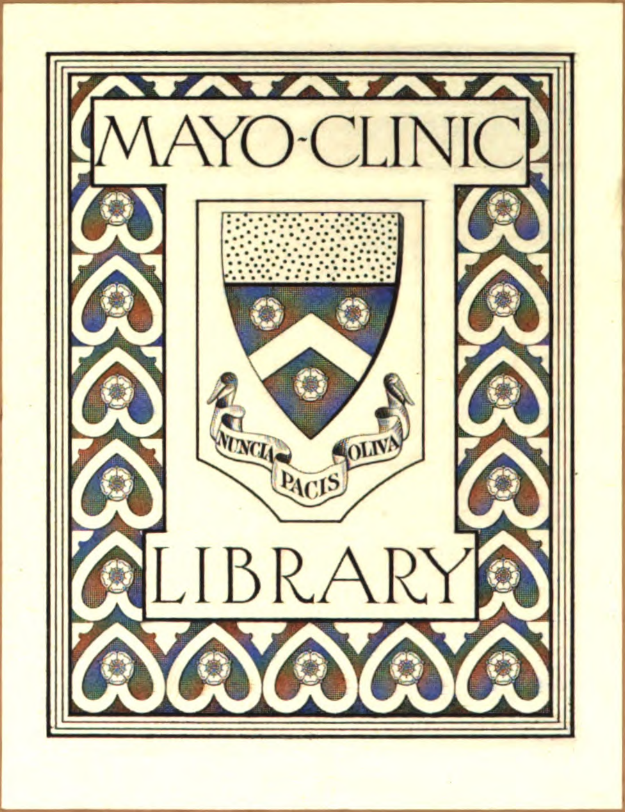
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NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

ROYAL INSTITUTION OF GREAT BRITAIN

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS

◆

VOLUME XXII

1917—1919

LONDON
PRINTED BY WILLIAM CLOWES AND SONS LIMITED
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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 26, 1917.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S.,
President, in the Chair.

Professor Gilbert Murray, M.A. LL.D. Litt.D. F.B.A.

Epicurean Philosophy.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, February 2, 1917.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
10 Vice-President, in the Chair.

Professor Charles S. Sherrington, M.D. LL.D. D.Sc. F.R.S.,
Fullerian Professor of Physiology.

Recent Physiology and the War.

This theme, kindly suggested by Professor Sir James Dewar, is sufficiently large to preclude more than a succinct treatment of some outstanding points in the time permissible in a single lecture. But these points are of considerable interest and have a more than fleeting importance.

The first is that of fatigue, its measurement and incidence in factory employees. The indices taken have been speed of output and quantity of output by groups of workpeople working under the conditions of a munitions factory. An inference of practical value drawn from the observations is that when the number of working hours per week was reduced from sixty-two to fifty-six the output actually increased. The reduction of the length of the working day by one hour per diem gave a rise of the total output of the week from an amount stated numerically as 6150 to an amount expressed as 6759. The output per hour increased 22 per cent. The kind of work in this case was "heavy," namely, deep screw-cutting by hand.

In another case, that of 200 women turning aluminium fuse-bodies, the reduction of the working hours per week from 68·2 to 60 notably increased the total output, and of course still more the rate of output. From these and other examples the lesson seems to be that there is for manual labour a certain length of working week, or working month, best suited for satisfactory production in permanence. The length varies with the class of the manual work. If a good efficiency is to be maintained in the factory this "most favourable" length of working month has to be followed. Before that it has to be found out and measured.

The next point raised was the influence of alcohol on the workers' output. The question has at present been attacked only in the laboratory so far as physiology is concerned. Physiological experiment shows that even a large single dose of alcohol—e.g. 40 cc.—has little or no effect upon the muscles *per se*, but that it does impair the working of the nervous system which actuates the muscles.

A suitable test in respect of the simplicity of the nervous centres involved in it is the knee-jerk. This is a familiar reaction to every physician; it is a reflex act, the spinal centre for which has been thoroughly investigated. The effect of a single dose of alcohol of 30 cc. quantity diluted with 120 cc. of water is to diminish and render sluggish the knee-jerk: the speed of the response is sometimes decreased by 9·6 per cent., the amplitude of the response lessened by 48·9 per cent. The greatest impairment of the reaction was noted about one hour after the dose.

Another test of the effect of alcohol on the musculo-nervous actions was furnished by a very simple voluntary act. The person subjected to the experiment was required to move one finger to and fro, that is, to bend and straighten the finger alternately, as rapidly as possible. The rate of movement was examined before and after taking a dose of 30 cc. alcohol diluted as above. This dose impaired the rate at which the oscillatory movement of the finger could be performed. The rate was diminished an hour after the dose by 8·9 per cent.

Such a movement is not well calculated to test that form of skill which consists in precision. Reasons were adduced for thinking that precision of movement is that aspect of a muscular act which will be most detrimentally interfered with by alcohol. The testing of alcohol effect by the ergograph seems to show that a moderate dose, say 30 cc. of alcohol, in a person accustomed to moderate use of alcohol, does not appreciably impair the power of the movement nor its resistance to fatigue. But the movements chosen as suitable for ergographic record are such as give little opportunity for the exhibition of precision or of skill of any kind.

The next point dealt with was the attempt to devise some fluid which can be injected to counteract the effect of severe loss of blood in the wounded. The properties desirable for the required fluid were shown to be: harmlessness in respect of avoidance of causing clotting in the circulation; restoration of the volume of the fluid in the circulation; maintenance of the due degree of viscosity of the circulating fluid, since on that factor depends the arterial and capillary pressure; and, finally, preservation of the balance between the osmotic pressure of the fluid inside the blood-vessels and outside in the tissues. It was shown that considerable success had been reached in this problem by the experiments of Professor Bayliss and others.

A final point dealt with was the treatment of tetanus by administration of "anti-tetanus serum." This serum is obtained from the blood of horses which have been subjected to gradually-increasing doses of tetanus-toxin, the poison produced by the tetanus-bacillus. The high efficiency of this anti-toxic serum when used as a prophylactic was first demonstrated on man on a large scale by its employment in the first autumn of this war. Curves illustrating the statistics were shown. The severe outbreak of tetanus which ensued in the troops at the outset of the campaign was checked and practically stopped almost instantaneously by the orders that every wounded man, as soon as possible after being wounded, that is to say, at the first field casualty-station, should receive a small injection of anti-tetanus serum from the immunized horse. But the efficacy of the serum when once signs of tetanus have appeared in the patient is far less satisfactory. The remainder of the Lecture was devoted to discussion of why this should be, and in what ways the difficulty may be, at least in part, overcome.

[C. S. S.]

GENERAL MONTHLY MEETING,

Monday, February 5, 1917.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. F.R.S., President, in the Chair.

E. Arthur Ashcroft,
T. Radford Thomson,
Charles F. Cross,

were elected Members.

Report from the Committee of Managers to the Members.

That the Members be informed that the Managers, as Trustees, after having taken the best advice, have already invested in War Stock as much as the available Funds permit them to do, and that they are about to transfer to the Treasury Authorities the Canadian Stock invested in the name of the Institution.

FACTS FOR THE MEMBERS.—During the years 1915–16 the following Sums have already been invested in the War Loan :—

	£	s.	d.
The Royal Institution.			
War Loan Stock 4½%	144	2	9
Exchequer Bonds 5%	1000	0	0
The Davy Faraday Research Laboratory.			
War Loan Stock 3½%	1108	4	7
ditto 4½%	527	11	6
Exchequer Bonds 5%	500	0	0
TOTAL	£3279	18	10

The Managers propose further to invest from the General Funds in the War Loan the Sum of £1000, and £500 from the Account of the Davy Faraday Research Laboratory.

The Managers also have deposited with the Treasury Authorities, under the provision of Scheme B, £3149 5*s*. 9*d*. Canada 4% Stock.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Memoirs of Department of Agriculture: Chemical Series, Vol. IV. No. 6. 8vo. 1916.

Pusa: Agricultural Research Institute, Bulletin, Nos. 62, 63 & 67; Report of Agricultural Research Institute, 1915–16. 8vo. 1916.

Accademia dei Lincei, Roma—Rendiconti: Classe di Scienze Morali, Storiche e Filologiche, Serie Quinta, Vol. XXV. Fasc. 5–6. 8vo. 1916.

Atti, Serie Quinta, Rendiconti: Classe de Scienze Fisiche, Mathematiche e Naturali, Vol. XXV. 2° Semestre, Fasc. 8–12. 8vo. 1916.

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American Chemical Society—Journal for Dec. 1916-Jan. 1917. 8vo.
Journal of Industrial and Engineering Chemistry for Dec. 1916-Jan. 1917. 8vo.
American Geographical Society—Geographical Review for Dec. 1916. 8vo.
American Journal of Physiology—Vol. XLII. Nos. 1-2. 8vo. 1917.
American Philosophical Society—Proceedings, Vol. LV. No. 7. 8vo. 1916.
Asiatic Society, Royal—Journal for Jan. 1917. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXVII. No. 1. 8vo. 1916.
Bankers, Institute of—Journal, Jan. 1917, Vol. XXXVIII. Part 1; Feb. 1917, Vol. XXXVIII. Part 2. 8vo.
Basel—Verhandlungen der Naturforschenden Gesellschaft in Basel, Band XXVII. 8vo. 1916.
Bevan, Rev. J. O., M.A. F.G.S. M.R.I. (the Author)—The Towns of Roman Britain. 8vo. 1917.
Bhownaggree, Sir Mancherjee M., K.C.I.E. (the Author)—The Verdict of India. 12mo. 1916.
Birmingham and Midland Institute—Report of Council for 1916.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXIV. Nos. 3-5. 4to. 1917.
British Astronomical Association—Journal, Vol. XXVII. No. 2. 8vo. 1916.
Brooklyn Institute of Arts and Sciences, The Museum of—Science Bulletin, Vol. II. No. 6. 8vo. 1916.
Cambridge Philosophical Society—Proceedings, Vol. XIX. Part 1. 8vo. 1917.
Canada, Department of Mines—Mines Branch: An Investigation of the Coals of Canada. 8vo. 1915.
Museum Bulletin, Nos. 23-24.
Canada, Royal Society of—Transactions, Third Series, Vol. X. Sept. 1916. 8vo.
Carnegie Institution—Contributions from Mount Wilson Solar Observatory, Nos. 115-123. 8vo. 1916.
Chemical Industry, Society of—Journal, Vol. XXXV. Nos. 22-23. 8vo. 1916; Vol. XXXVI. No. 1. 8vo. 1917.
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Comité d'Administration de France, Maroc—Revue Mensuelle, No. 1. 4to. 1816.
Consolo, Enrico (Banca Commerciale Italiana)—The War in Italy. Vols. I.-III. 4to. 1916.
Cutlers, The Worshipful Company of—History of the Cutlers' Company of London. By Charles Welch, F.S.A. Vol. I. 8vo. 1916.
Devonshire Association—Report and Transactions, Vol. XLVIII. July 1916. 8vo. 1916.
East India Association—Journal, New Series, Vol. VIII. No. 1. 8vo. 1917.
Editors—Aeronautical Journal for Oct.-Dec. 1916. 8vo.
Amateur Photographer for Dec. 1916. 4to.
American Journal of Science for Dec. 1916-Jan. 1917. 8vo.
Athenæum for Dec. 1916-Jan. 1917. 4to.
Author for Dec. 1916-Jan. 1917. 8vo.
Chemical News for Dec. 1916-Jan. 1917. 4to.
Chemist and Druggist for Dec. 1916-Jan. 1917. 8vo.
Church Gazette for Dec. 1916-Jan. 1917. 8vo.
Concrete for Dec. 1916-Jan. 1917. 8vo.
Dyer and Calico Printer for Dec. 1916-Jan. 1917. 4to.
Electrical Engineering for Dec. 1916-Jan. 1917. 4to.
Electrical Industries for Dec. 1916-Jan. 1917. 4to.
Electrical Review for Dec. 1916 Jan. 1917. 4to.

Editors—continued
Electrical Times for Dec. 1916-Jan. 1917. 4to.
Electricity for Dec. 1916-Jan. 1917. 8vo.
Electric Vehicle for Dec. 1916. 8vo.
Engineer for Dec. 1916-Jan. 1917. fol.
Engineering for Dec. 1916-Jan. 1917. fol.
Ferro-Concrete for Dec. 1916-Jan. 1917. 8vo.
General Electric Review for Dec. 1916-Jan. 1917. 8vo.
Horological Journal for Dec. 1916-Jan. 1917. 8vo.
Illuminating Engineer for Dec. 1916. 8vo.
Journal of Physical Chemistry for Dec. 1916. 8vo.
Journal of the British Dental Association for Dec. 1916-Jan. 1917. 8vo.
Junior Mechanics for Dec. 1916-Jan. 1917. 8vo.
Law Journal for Dec. 1916-Jan. 1917. 8vo.
Marine Engineer for Dec. 1916-Jan. 1917. 8vo.
Marine Magazine for Dec. 1916-Jan. 1917. 4to.
Model Engineer for Dec. 1916-Jan. 1917. 8vo.
Musical Times for Dec. 1916-Jan. 1917. 8vo.
Nature for Dec. 1916-Jan. 1917. 4to.
New Church Magazine for Dec. 1916-Jan. 1917. 8vo.
Page's Weekly for Dec. 1916-Jan. 1917. 8vo.
Physical Review for Dec. 1916. 8vo.
Power for Dec. 1916-Jan. 1917. 8vo.
Power User for Dec. 1916-Jan. 1917. 8vo.
Russian Co-operator for Dec. 1916. 8vo.
Science Abstracts for Dec. 1916-Jan. 1917. 8vo.
Tchèque, La Nation, for Dec. 1916. 8vo.
War and Peace for Dec. 1916-Jan. 1917. 8vo.
Wireless World for Dec. 1916-Jan. 1917. 8vo.
Zoophilist for Dec. 1916-Jan. 1917. 8vo.
Electrical Engineers, Institution of—Vol. LV. Nos. 261-262. 8vo. 1916.
Faraday House—Journal, Vol. VII. No. 2. 8vo. 1917.
Florence Biblioteca Nazionale Centrale—Bollettino, Nos. 192-193. 8vo. 1917.
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Geographical Society, Royal—Journal, Vol. XLVIII. No. 6. 8vo. 1916; Vol. XLIX. Nos. 1-2. 8vo. 1917.
Geological Society of London—Quarterly Journal, Vol. LXXI. Part 4. 8vo. 1917.
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Imperial Institute—Bulletin, July-Sept. 1916. 8vo.
Jones, Daniel, M.A. M.R.I. (the Author)—A Sechuana Reader. 8vo. 1916.
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Kyōto Imperial University—Memoirs of College of Engineering, Vol. I. Nos. 6-7. 4to. 1916.
Memoirs of College of Science, Vol. I. Nos. 8-10. 1916. 8vo.
Literature, Royal Society of—Transactions, Vol. XXXIV. 8vo. 1916.
Report and List of Fellows, 1916. 8vo.
London County Council—Gazette for Dec. 1916-Jan. 1917. 4to.
Manchester Steam Users' Association—Boiler Explosions Acts, 1882 & 1890. Reports, Nos. 2327-2387. 4to. 1916.
Meteorological Office—Monthly Weather Reports for Nov.-Dec. 1916. 4to.
Weekly Weather Reports for Dec. 1916-Jan. 1917. 4to.
Daily Readings for Oct. 1916. 4to.
Geographical Journal for Jan.-Feb. 1915. 4to.
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Nova Scotian Institute of Science—Proceedings and Transactions of the, Vol. XIV. Part 2. 8vo. 1916.

Numismatic Society, Royal—Numismatic Chronicle, Fourth Series, Part 3, No. 63. 1916. 8vo.

Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for Sept.-Dec. 1916. 4to.

Peru, Corps of Mining Engineers—Bulletin, No. 82. 8vo. 1916.

Pharmaceutical Society of Great Britain—Journal for Dec. 1916–Jan. 1917. 8vo. The Calendar, 1917. 8vo.

Photographic Society, Royal—Journal, Vol. LVII. No. 1. 8vo. 1917.

Physical Society of London—Proceedings, Vol. XXIX. Part 1, Dec. 1916. 8vo.

Post Office Electrical Engineers, Institution of—Nos. 68–69. 8vo. 1916.
Journal, Vol. IX. Part 4, Jan. 1917.

Quekett Microscopical Club—Journal, Series 2, Vol. XIII. No. 79. 1916. 8vo.

Rockefeller Institute for Medical Research—Studies, Vol. CCXXIV. 8vo. 1916.

Rome, Ministry of Public Works—Giornale del Genio Civile for July–Sept. 1916. 8vo.

Royal Colonial Institute—United Empire, Vol. VII. No. 12. 8vo. 1916; Vol. VIII. No. 1. 8vo. 1917.

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Royal Society of Arts—Journal for Dec. 1916–Jan. 1917. 8vo.

Royal Society of London—Proceedings, Series A, Vol. XCII. Nos. 643–645; Vol. XCIII. No. 646; Series B, Vol. LXXXIX. Nos. 616–617. 8vo. 1916.
Transactions, Series B, Vol. CCVIII. No. 352. 4to. 1917; Series A, Vol. CCXVII. No. 551. 4to. 1917.

Sanitary Institute, Royal—Journal, Vol. XXXVII. No. 4, Dec. 1916. 8vo.

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Arsbok, 1916; Handlingar, Band LV. Nos. 1–6. 4to. 1916.

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Experiment Station Record, Vol. XXXV. Nos. 6–8. 8vo. 1916.

United States Department of Commerce and Labour—Bulletin of Bureau of Standards, Vol. XII. No. 4; Vol. XIII. Nos. 1–2. 8vo. 1916.

United States Patent Office—Official Gazette, Vol. CCXXXIV. No. 1, Dec. 1916. 8vo. 1917.

Zoological Society of London—Proceedings, Part 4, Dec. 1916. 8vo. 1916.

WEEKLY EVENING MEETING,

Friday, February 9, 1917.

The Right Hon. Lord Rayleigh, O.M. P.C. D.C.L. F.R.S., in the Chair.

Daniel Jones, M.A. M.R.I.

Experimental Phonetics and its Utility to the Linguist.

The art of speaking a foreign language demands (among other things) an ability to perform all kinds of difficult movements with the tongue and other parts of the speech-mechanism. Such ability may be acquired by the learner, if he is provided with precise instructions as to what he must do. It is the function of the phonetician to supply these instructions.

Instructions as to how to pronounce must, in order to be efficacious, be based on accurate analysis of the pronunciation. Many of the facts of pronunciation can be ascertained by direct observation (by auditive, visual, tactile, and muscular sensation) on the part of those who have a specially-trained ear and a highly-developed control over their vocal organs. These methods are extremely important, and no satisfactory analysis of a language can be made without them. Other methods, however, may be used to supplement these, namely mechanical analysis by means of specially-designed apparatus. Analysis of this kind constitutes the branch of phonetics known as experimental phonetics. It is these mechanical aids to analysis which form the subject of the present discourse.

It will be well to give first a few examples, to show how information regarding *tongue-positions* may be ascertained experimentally.

One way of getting information is that known as *Palatography*. It consists in using a special kind of artificial palate, in order to find out what parts of the roof of the mouth are touched by the tongue in the production of different speech-sounds.

The requirements of this special type of artificial palate are that it should be very thin, should fit very accurately, should be dark-coloured, and should cover the whole of the hard palate, alveolars, and the under-side of the upper front teeth. Such palates may be made of vulcanite, or metal, or other substances.

When the palate is to be used, it is dusted over with powdered chalk; it is then inserted into the mouth; the sound to be studied is pronounced, and the palate is taken out. It will be found that the chalk has been removed by the tongue at every point which the tongue has touched in articulating the sound. So the areas touched by the tongue appear dark, while the parts of the palate which are not touched remain white.

The shapes of the dark areas may be recorded by photography if desired, but it is generally sufficiently accurate, and a good deal more convenient, simply to copy the dark areas on to a previously-prepared outline diagram of the palate. (The result is, of course, a projection of the true shape.) The finished diagrams are called *palatograms*. Palatograms will be found to corroborate observations of tongue-positions made by other methods.

[Numerous examples of palatograms were shown on the screen; Figs. 1 and 2 are specimens.]

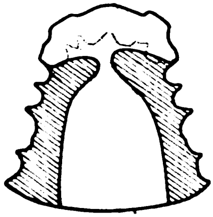

Fig. 1.
Palatogram of *s*.

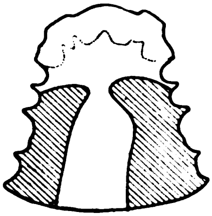

Fig. 2.
Palatogram of the English Sound of *sh*.

We will now turn to methods of ascertaining the *shapes* assumed by the tongue in the articulation of speech-sounds, and more particularly the shapes of a section of the tongue down the mesial line, and their relations to the centre-line of the palate.

One method of ascertaining these shapes was invented by Dr. E. A. Meyer, of Stockholm. It consists in using an artificial palate down the middle line of which are fixed some lead threads which hang vertically. These threads are of such a thickness that the pressure from the tongue will bend them when a speech-sound is produced; but they are strong enough to remain in the position into which they are pushed. So that if the palate is taken out of the mouth after pronouncing a speech-sound, the lead wires show the outline of the tongue-position compared with that of the palate. There is a means of transferring these outlines to paper.

A second apparatus for obtaining similar results is the "mouth-measurer" invented by H. W. Atkinson.[*] There is a tube of the shape ACB, shown in Fig. 3, and inside the tube is a wire which can be pushed along (by means of the handle D) and made to project to different lengths from the end of the tube. A projecting piece of metal, called a "tooth-stop" (E), is attached to the tube; it can be fixed at various points. FGH is a wire handle.

---

[*] Obtainable from H. W. Atkinson, Esq., West View, Eastbury Avenue, Northwood, Middlesex. (Price 5*s*. 6*d*. for set of two Mouth-Measurers, with accessories.)

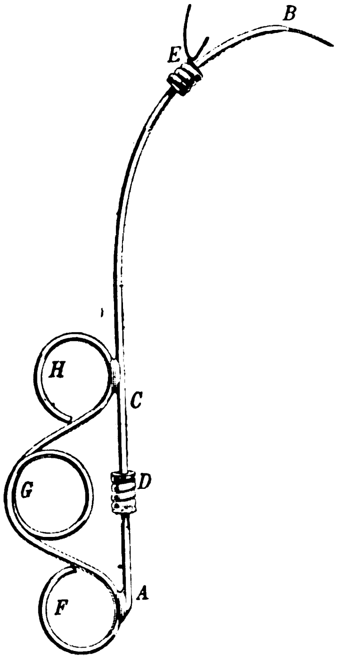

Fig. 3.
Atkinson's Mouth-Measurer.
ACB tube, D handle of wire,
E tooth-top, FGH handle.

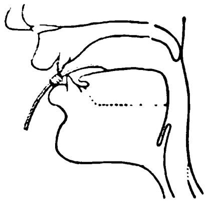

Fig. 4.
Atkinson's
Mouth-Measurer
in Position.

To use the instrument, it is placed in the mouth either in the manner shown in Fig. 4, or else so that the tube is in contact with the teeth at the tooth-stop and also in contact with some point of the palate (the position of the apparatus depending on the nature of the sound to be analysed). The wire is then pushed along until the end of it is felt to touch the tongue. The instrument is withdrawn and applied to a previously-prepared diagram of the shape of the observer's palate. The position of the end of the wire is then marked on the paper.

Further observations are then taken with the tooth-stop fixed at other points. In this way the positions of other points of the surface of the tongue are ascertained. In the end we get on our paper a series of, say, ten or more points which show with fair accuracy the shape of the most important part of the tongue.

Fig. 5 shows specimens of results obtained by this means. They were prepared by Mr. Atkinson, and are reproduced here by his kind permission.

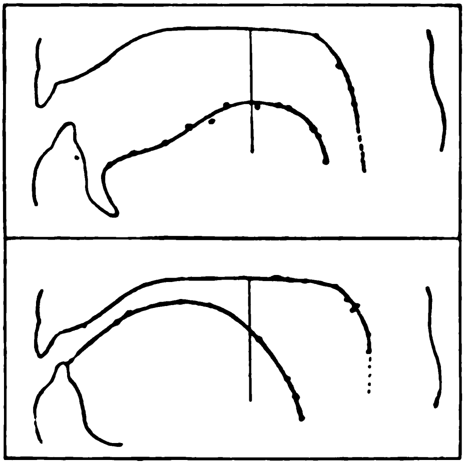

FIG. 5.—TONGUE-POSITIONS OF THE ENGLISH VOWELS IN *bath* AND *beat* AS ASCERTAINED BY ATKINSON'S MOUTH-MEASURER.

A third method of obtaining sectional diagrams of tongue-positions is X-ray photography. In order to get good results by this process it is necessary to make use of some opaque substance to show the outline of the tongue. The plan which has given the most successful results is to place on the tongue a little chain of small lead plates. (This plan was originally devised by Dr. E. A. Meyer.)

Figs. 6 to 10 are photographs of this description, taken specially for this occasion by Dr. H. Trevelyan George, M.R.I., who has displayed much ingenuity and patience in getting over the numerous difficulties which present themselves in the course of work of this nature.

Another element of speech which can be successfully studied by the methods of experimental phonetics is the vibration of the vocal chords. Some speech-sounds (e.g. normal *v* or *z*) are accompanied by vibration of the vocal chords, others (e.g. *f*, *s*) are not; others again are accompanied by vibration during a part of their length. It is important for linguistic purposes to ascertain with accuracy the precise points where vibration of the vocal chords begins and ends in connected speech.

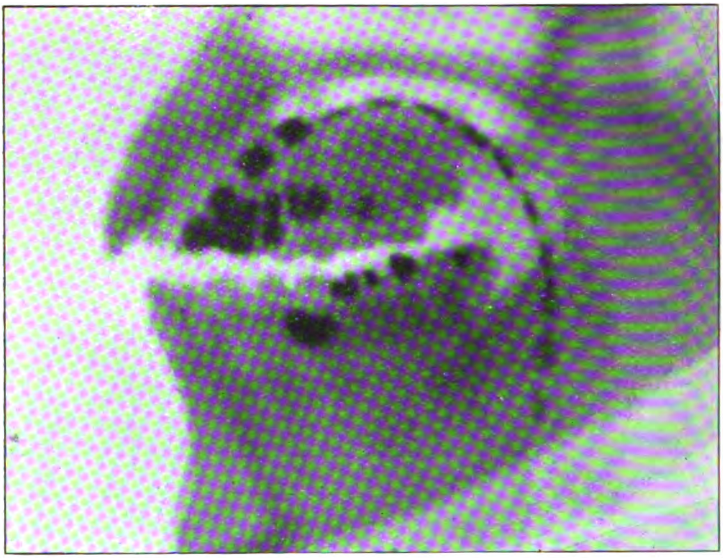

FIG. 6.—X-RAY PHOTOGRAPH OF CARDINAL VOWEL *i* (AS IN FRENCH).

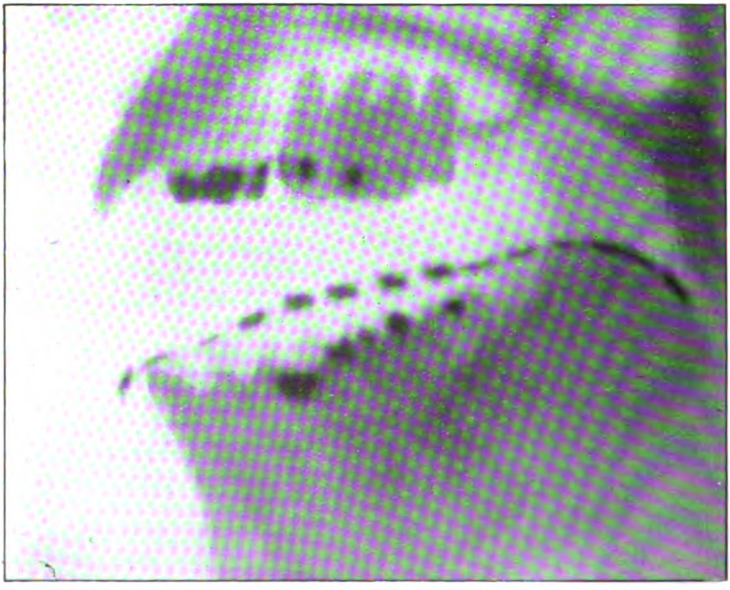

FIG. 7.—X-RAY PHOTOGRAPH OF CARDINAL *a*.

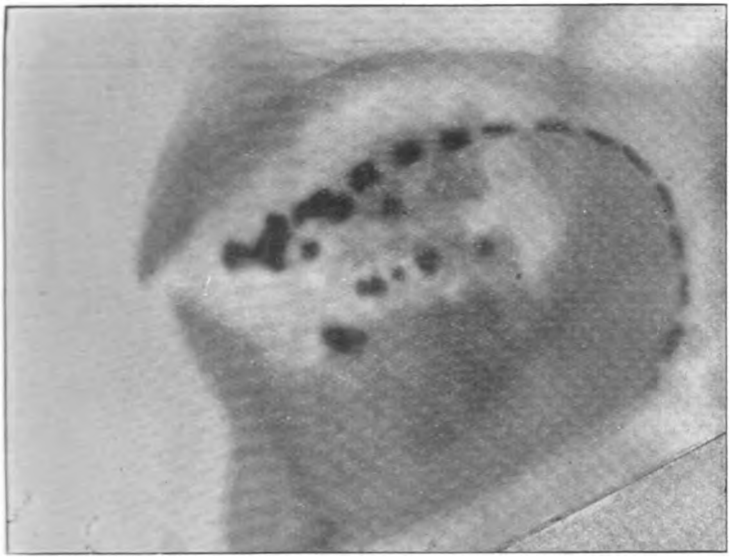

Fig. 8. – X-Ray Photograph of Cardinal *u*.

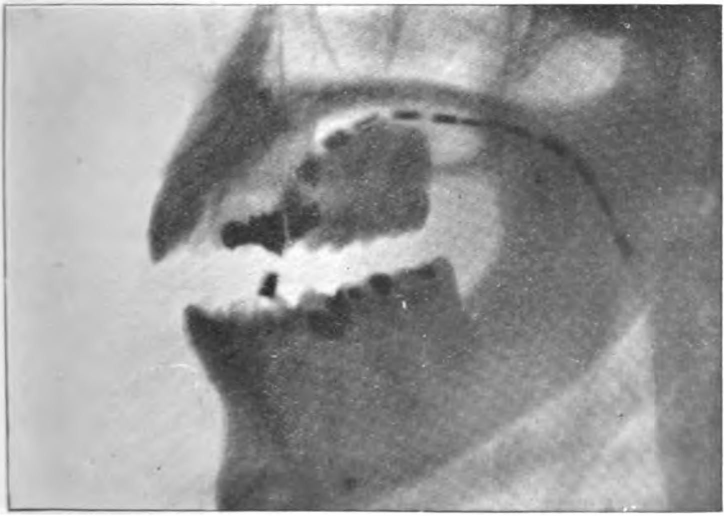

Fig. 9.—X-Ray Photograph of *k* as in *cave*.

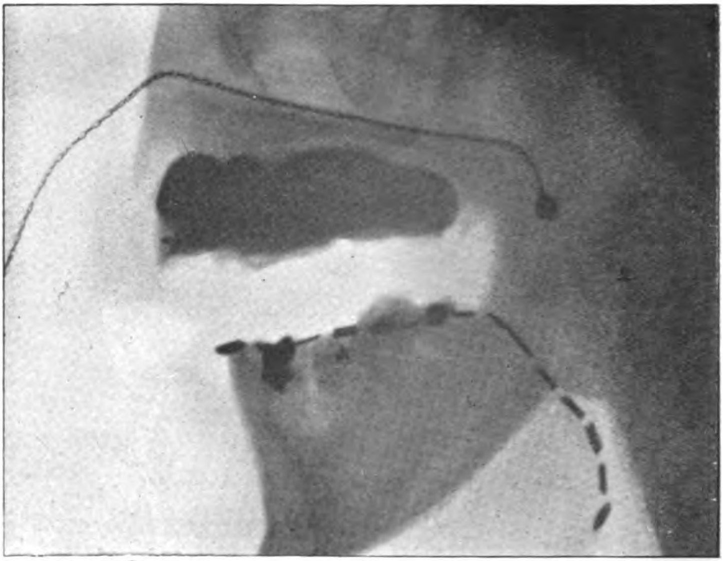

Fig. 10.—X-Ray Photograph of Welsh *o* (as in *ton* "Wave").

Said by Mr. Stephen Jones, Assistant for Experimental Phonetics at University College, London. Tongue-position shown by lower chain. The upper chain passes through the nose, and shows the shape of the upper side of the soft palate.

There are several ways of recording mechanically the presence or absence of voice. The method which gives the most satisfactory results from the point of view of the linguist consists in using a kymograph fitted with one or more tambours of Marey's model. The Marey tambour is a mechanism by which vibrations of air are communicated to an elastic membrane and thence to a very light recording needle. The vibrations of the recording needle are generally recorded on a revolving drum covered with smoked paper, or some similar contrivance. The most useful type of tambour is one with a perished rubber membrane, 3 cm. in diameter.

Air-waves set up by speech may be communicated to a tambour (1) from the mouth (by speaking into a mouth-piece); (2) from the nose (by using a "nasal olive"); or (3) from the outside of the larynx (by using a "larynx capsule.")

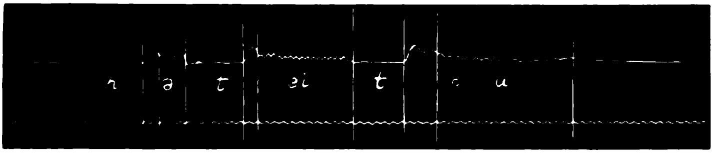

FIG. 11.—MOUTH-TRACING OF *potato*. Below is a tracing of a tuning-fork giving 100 vibrations per second.

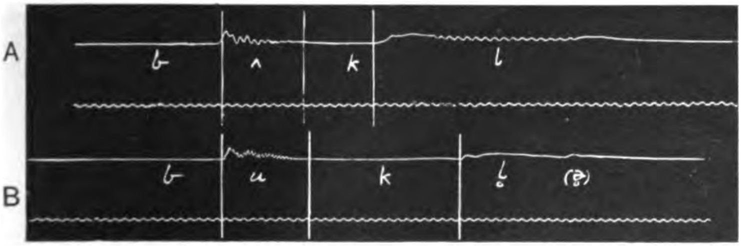

FIG. 12.—MOUTH-TRACINGS OF (A) ENGLISH *buckle* (MALE VOICE) AND (B) FRENCH *boucle* (FEMALE VOICE).
Note the unvoicing of the French *l*, and the length of the *k*.

[A kymograph with all fittings was shown. A kymographic tracing of the word *put* was made on a piece of smoked glass and shown on the screen.]

The nature of these kymographic speech-curves can be demonstrated to a large audience by substituting a very small mirror for the tambour-style and directing a powerful beam of light on to it in a darkened room. This beam of light must be reflected from the tabbour on to a revolving mirror and thence on to the screen. The virations of the tambour membrane are then seen as curves on the screen.

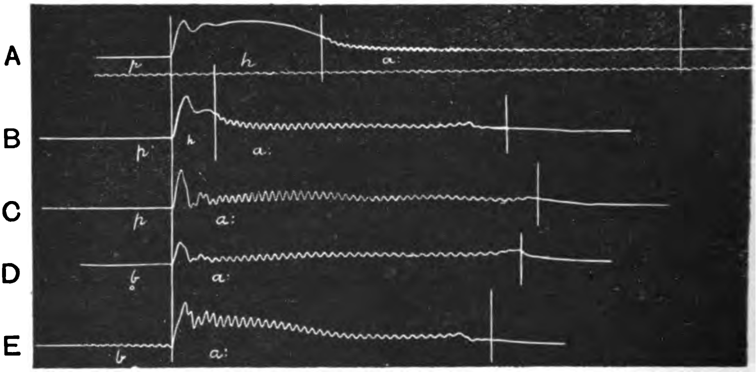

Fig. 13.—Mouth-Tracings of (A) Fully-aspirated *p*; (B) Partially-aspirated *p*; (C) Unaspirated *p*; (D) Unvoiced *b*; and (E) Fully-Voiced *b*—each followed by the vowel *a*.

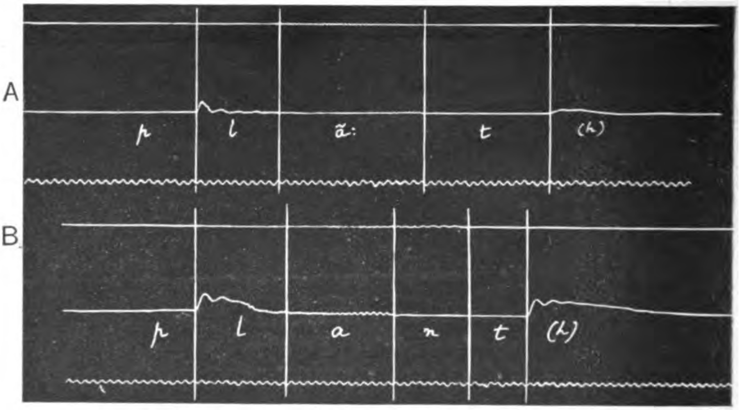

Fig. 14.—Simultaneous Mouth- and Nose-Tracings of (A) French *plante* (Female Voice); (B) English *plant* (Male Voice). Note the absence of *n* in French.

[This experiment was performed, and the differences in the effects of various speech-sounds were shown. Special attention was called to the variations in the size of the vibrations when vowels were pronounced on different pitches of the voice.

Numerous lantern-slides illustrating kymographic tracings were then shown. Some of these tracings are reproduced in Figs. 11 to 15. Such tracings are chiefly useful (1) for detecting the presence or absence of voice; (2) for detecting the presence or absence of nasality; (3) for measuring the lengths of sounds; and (4) for calculating the pitch of the voice.]

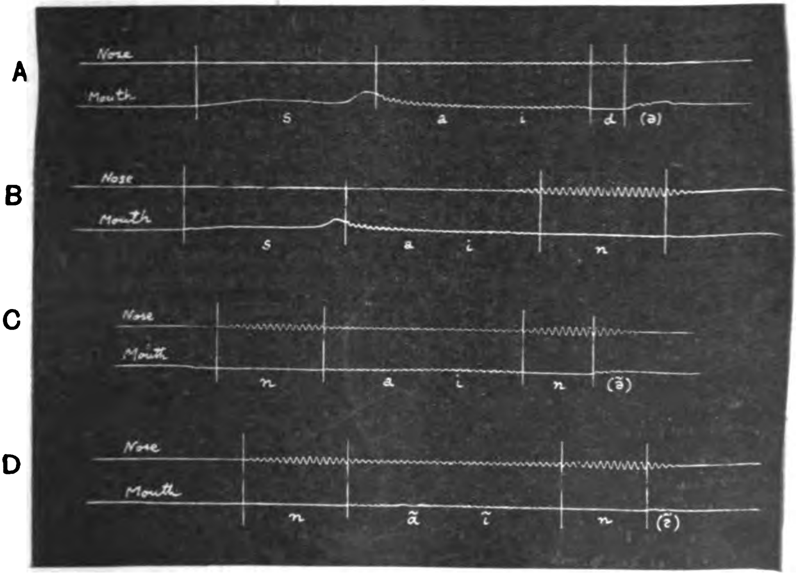

FIG. 15.—SIMULTANEOUS MOUTH- AND NOSE-TRACINGS OF (A) *side*; (B) *sign*; (C) *nine*; AND (D) *nine* PRONOUNCED IN COCKNEY-FASHION.

Note the differences in the nose-tracings.

The above examples show to what extent experimental phonetics may be useful to the language learner. It furnishes him with much of the information he wants in regard to pronunciation. The practical linguist should make these ascertained facts the basis of his study of the pronunciation of the language he is learning. He will be able to infer from them how he must proceed in order to get his own organs of speech to perform the movements required by the foreign language.

In conclusion, it may be as well to point out that as these scientific methods of analysis are useful to the linguist, so also the accomplishments of the linguist are sometimes found to have their uses to the man of science.

Thus it is possible by means of a speech process to demonstrate in a remarkable way the existence of harmonics in a musical note, to show, for instance, that if the note *c* is sung, there is sounding simultaneously the well-known series of harmonics c', g', c'', e'', g'', etc. This fact is made evident by putting the mouth into a series of positions which will act as resonators and reinforce different harmonics one after the other. If only one position is taken up by the mouth, some harmonic or other is necessarily reinforced, but it is extremely difficult to detect which. But by making rapid changes from one mouth-position to another, the successive harmonics become clearly audible *by contrast*. The speech-movement which makes these harmonics come out most clearly is to start by holding the tongue in the position of the English sound of *ng* and rounding the lips and gradually separating them. At close quarters the effect is that of an arpeggio played on a tiny harp. If the voice-note is changed, the same arpeggio is heard in a different key.

[This experiment was performed.]

This phonetic experiment may or may not prove to have some direct value in the direction of elucidating problems of sound-quality, but at any rate it is useful as a practical demonstration of the presence of harmonics in a musical sound.

[D. J.]

WEEKLY EVENING MEETING,

Friday, February 16, 1917.

THE RIGHT HON. LORD WRENBURY, P.C., Vice-President, in the Chair.

VERY REV. H. HENSLEY HENSON, Dean of Durham.

Authors' Dedications in the Seventeenth Century.

DEDICATIONS now mostly nothing more than personal compliments, but once had a greater importance. In some cases they possess considerable value, both historical and biographical. In seventeenth century the "dedicatory epistle" often a concise statement of author's argument, reasons for writing, and circumstances. Hardly excessive to say that the introductory compositions are the only portions of some famous books now known generally. Literary finish secured by the desire to make a favourable impression. Reasons why authors sought the patronage of prominent persons. Fuller's view and practice. Financial policy latent in dedications explains much of the adulation which impairs their credit and literary quality. Dedications dictated by personal friendship have no taint of flattery or self-seeking. Extraordinary dedications are the satirical (e.g., George Withers and Lord Herbert), and polemical (e.g., Milton, Heylyn, Hall). Herbert and Bunyan are examples of edifying dedications, and stand by themselves Selden's dedication of "Titles of Honour" to Mr. Edward Heyward gives his opinion of conventional dedications. The "Dedicatory Letter" prefixed to the republication of this treatise is almost an essay on dedication. Selden, like Fuller, divides authors' dedications into three classes—instruction, censure, love and honour. We may accept a three-fold classification—personal, official, financial. Selden was exceptional as a man of comparative wealth, but he had experience in connexion with his "History of Tithes" of the risks of independence. His servile description of James I. may stand beside Johnson's interview with George III. Choice of person to whom a book should be dedicated was determined by a variety of considerations—personal inclination, obvious fitness, official propriety, polemical effect, calculations of prudence, financial interest. A dedication implied patronage, which was indispensable in that age, when few authors made money by their works. Hall "wrote books to buy books"; Fuller boasted that "no stationer had lost by him." Both were exceptionally successful writers. Mostly the author depended on the patron's complimentary present. Hall provides examples of many varieties of dedication. He links together theology and literature. "The children of the bondman are the goods of the parent's master" is his principle in dedicating "Heaven upon Earth" to his father's employer, the Earl of Huntingdon. As a royal chaplain, he naturally dedicated many works to Prince Henry and James I. These dedications illustrate the incidental historical value of such compositions. Fuller's numerous dedications are filled with side-lights on men and events. An excellent example is his dedication of the Fifth Book of his "Church History" to Cranfield, Earl of Middlesex. In varying measure the same kind of value attaches to all Fuller's numerous dedications. The extent of his acquaintance is indicated by the number and variety of his patrons, which Mr. Brewer holds to be also the disproof of the imputation of "intentional dishonesty, designed to curry favour with the reigning powers," which was brought against him in his lifetime and since. His fondness for infant-patrons, and defence of it. Fuller's dedicatory epistles are fullest of personal interest; Jeremy Taylor's have the highest literary quality. These were sometimes in length and form rather essays than epistles, e.g., the dedication of "The Liberty of Prophesying." He regards the epistle dedicatory as far more than a merely complimentary composition; and treats it rather as a modern writer would treat his preface or introduction. His dedication of the first edition of "The Great Exemplar" to the second Lady Carbery is a curious example of tactfulness. In the dedication of "A Collection of Polemical and Moral Discourses" to Lord Hatton in 1657, Taylor discusses patrons and books. Charles II., an edifying figure in dedicatory epistles. His romantic history and the re-establishment of the national hierarchy as a consequence of his restoration may explain this. Robert Barclay's dedication of the "Apology," 1675, a notable exception to the general tone of Caroline dedications. Possibly the "Apology" may have reached the King through the Duke of York, with whom Barclay was acquainted. Heylyn and Burnet tried to connect the Restoration with their polemical object in writing. A selection of "dedicatory epistles" of the seventeenth century, edited by a competent historian, would be both serviceable to students and interesting to the general reader.

[H. H. H.]

WEEKLY EVENING MEETING,

Friday, February 23, 1917.

[No Discourse Delivered.]

WEEKLY EVENING MEETING,

Friday, March 2, 1917.

Sir William Phipson Beale, Bart., K.C. M.P.,
Vice-President, in the Chair.

Charles F. Cross, B.Sc. F.C.S. V.P.I.C.

Cellulose and Chemical Industry (1866-1916).

Cellulose in the natural order ranks with the air-gases, and water as a primary substance, or, in the sense of the Greek cosmology, an "element."

In the affairs of "the world," that is, man's world, it plays a similar predominant part by reason of its original qualities of structure and of resistance as a chemical individual to oxygen and water. Under chemical treatment there results a development of structural adaptability through the plastic properties of characteristic synthetic derivatives; and in the special properties of the cellulose nitrates there is realized the many-sided technical ideal of explosive and the indispensable condition *qua* matter of modern warfare.

The chemical industrial developments of cellulose are characteristic of the modern age, which in this section of technology dates from 1866. At this date the necessity of meeting the progressive consumption of paper by a supply of original raw cellulose material led to the introduction of esparto grass by Thos. Routledge, and to the investigation of wood material as the most massive form and source of cellulose. As the result of the pioneer work of an Anglo-Swedish group, of which the late C. D. Ekman was a prominent member, and for which his English collaborators (Messrs. Thomson, Bonar and Co., and George Fry, F.L.S., of Berwick-on-Tweed) supplied the means, the business organization, and a considerable contribution to the technical scientific basis of investigation, the wood pulp (cellulose) industry was launched.

It was considerably developed by the technical improvements introduced by our countryman, Edward Partington, now Lord Doverdale, who also brought to bear on the industry organizing and business capabilities of a high order; the results, which have been cumulatively successful, are known to the world.

In these processes of resolving raw material into paper-maker's "pulps," the cellulose, as the chemically inert or non-reactive basis of the raw materials, resists the severe treatments required to attack the non-cellulose components. The esparto compound celluloses are resolved by alkaline treatment (caustic soda solution at 120°–130°), the wood substance (ligno-cellulose) by treatment with acid compounds (bi-sulphites) in solution at 140°–160°.

The soluble by-products, derivatives of the non-cellulose complex, amounting in either case to 50 per cent. of the original raw material, are in effect waste products. In the case of the esparto they are burned incidentally to the recovery of the soda; but the "wood liquor" by-product is still in the main a waste, and a colossal one.

The "positive" chemical technology of cellulose is necessarily based upon its reactivity, and the industries which exploit the reaction changes of cellulose are of widely divergent character; those of Group A are based upon reactions of decomposition; those of Group B are based upon the properties of synthetical derivatives.

Of Group A, the resolution of cellulose into sugar (dextrose) by acid hydrolysis is the basis (1) of a process for the preparation of industrial (ethyl) alcohol from wood, and more economically from wood wastes. According to our latest information the technical difficulties presented by this apparently simple process have been so far overcome that the alcohol is produced on the large scale at a cost of 2½*d*. per gallon, exclusive of the small cost of the waste wood material. Under these most favourable conditions, however, the yield of alcohol is only 5 per cent. of the weight of the wood substance, or say 8 per cent. of its cellulose content, leaving therefore a very large item of final waste to the debit account of the chemist.*

(2) Sawdust, or other wood waste, fused—i.e. heated at high temperatures—with the caustic alkalis is destructively oxidized, and the main product of the oxidation is oxalic acid; this process is in effect the main source of the acid, as an industrial product.

(3) Waste wood subjected to destructive distillation in closed retorts is resolved into gaseous and volatile liquid products, with a solid residue of charcoal or pseudo-carbon. The liquid products contain acetic acid, acetone, and methyl-alcohol as main constituents, and have been the main source of supply of these chemical individuals, which, as reagents especially, are indispensable in modern chemical industry.

In this direction, however, cellulose material and its wasteful treatment by destructive distillation are being supplanted by a direct and controlled synthesis from calcium carbide, acetylene being quantitatively transformed into acetaldehyde, and this into acetic acid. In this new industry the pioneers in this country are the British Cellulose and Chemical Manufacturing Company, Spondon, Derby.

(4) Cellulose heated in full contact with oxygen is burned with the familiar flame combustion; but from all the natural structural forms there is a residue of inorganic matter (ash) which when present

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* See Journ. Soc. Chem. Ind., 1900–1916.

in important proportion preserves the general structural details of the original. This intimate combination of cellulose with inorganic matter is a characteristic property of colloids of which cellulose is the prototype. It is the basis of innumerable processes incidental to the dyeing and colouring of cellulose fabrics and tissues. But the production of a combustion skeleton for utilization as an industrial product is a modern and noteworthy realization of a primary cellulose quality "strong even in death."

The familiar incandescence gas mantle is the skeleton of a textile material impregnated as such, or as we may say, "in the flesh," with colloidal thorium and cerium oxides, and afterwards cremated.

Of the cellulose industries of Group B, those (1) based on the nitric esters are of preponderating importance, for they include the production of the modern military explosives, and they connote developments in pure and applied science peculiarly characteristic of the age. Cellulose nitrate not only fulfils the ideal of chemical effect, i.e. total conversion of solid into gaseous matter, at maximum increase of volume (or pressure) further increased by the temperature of combustion, but is a stable form of this high chemical potential, and further owing to its colloidal plastic properties can be moulded into any desired form, and is therefore able to meet the most exacting specification of ballistic requirements. There is no doubt that mankind is directly indebted for its "smokeless" powders and the basis of the vast developments of modern military power to the pioneer work of Alfred Nobel and his collaborators.

The lower degrees of "nitration" of cellulose are represented by products which are the basis of "celluloid," and the celluloid industries connote the production of a large number of familiar objects both useful and ornamental, and a progressively increased production from the date of the pioneer work of the American inventor, Hyatt (1869).*

Celluloid realizes a very high order of plasticity, and it is this structural potentiality which enables it to maintain its industrial leadership in spite of its disadvantages of high inflammability, frequently and tragically manifested.

(2) The analogues of the nitric esters are the acetic esters of cellulose, derivatives which are necessarily colloids with plastic capabilities, water-resistant as are the nitrates, and with a higher order of resistance to heat (unchanged at 200° C.), and the ordinary inflammability of organic substances. Important uses of the cellulose acetates already established are: (*a*) in aeroplane construction, for treating to render taut and impermeable the textile fabric which constitutes the air-resistant surface of the wings; (*b*) in the form of film, as the "emulsion-film" support in photographic work, notably for the preparation of the continuous cinema picture; (*c*) for a

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* See Journ. Soc. Chem. Ind., xxxiii. (1914), p. 225.

varnish for metallic surfaces, and notably for electrical insulation and applications which depend upon exceptionally low inductive capacity.

In the above brief account of the cellulose nitrates and acetates there is the implicit suggestion of the production of artificial textile threads by drawing the solutions of these bodies through orifices of suitable form and dimensions, such that after removal of the solvent liquid the re-solidified and structureless ester constitutes a cylinder of hyaline material of uniform diameter.

The industrial production of artificial threads, and notably of the "silk" or lustra cellulose, we owe to the pioneer enterprise and purposeful tenacity of a French technologist, H. de Chardonnet. But cellulose nitrate as *matière première* has many and obvious defects; hence the alternative plastic forms or derivatives of cellulose have progressively displaced the original industrial product, which had a brilliant career of success in the early years of this century.

There are three main rival processes of which the underlying principles are common. They are expressed in the stages or phases of the process:—

(1) Cellulose is transformed by reaction into a synthetic derivative—dissolved as such to an 8 per cent. solution (calculated as cellulose), filtered and continuously projected, or drawn through fine tubes or orifices, into a solidifying or setting medium.

(2) The thread as a unit filament (monofil, crin, crinole), or multiple group of twisted filaments (artificial silk), is a cellulose ester or other derivative, and is chemically treated to remove combined groups, or impurities as by-products of decomposing reactions; the thread substance is cellulose, reappearing as such after the cycle of changes.

The three cellulose derivatives on which these important industrial processes are based are: (1) The nitric esters; drawn or "spun," as ether-alcohol solution (Chardonnet process).

(2) The ammonia-soluble cellulose-hydrate-copper-hydroxide compounds (Cuprammonium process).

(3) The xanthic or sulpho-carbonic ester, synthesized in two stages: (*a*) mercerization of the cellulose; (*b*) combination of the alkali cellulose with carbon disulphide, and solution of the product in water (Viscose process).

Of these the Viscose process is now predominant by reason of its advantages, technical and economic. It will be noted that the material "elements" of the industry are cellulose, common salt, carbon and sulphur, than which no simpler terms could be devised, even as an a priori proposition.

Of other industrial applications of the Viscose process, the production of transparent film in continuous length is a variation only of the process above described, the liquid Viscose being drawn through a controlled adjustable slit of 1·5 m. width, into a setting solution or precipitant, which converts it at once into the solid state, but retaining 80 per cent. of water. This hydrated product is purified by successive treatments, and is finally dehydrated and dried with a shrinkage of 33 per cent., the 1·5 m. of hydrated xanthate, as first precipitated, being reduced to a width of 1 m. in the finished cellulose film.

This is a noteworthy achievement in chemical engineering, and is entirely due to the inventive persistence of the personnel of the Société Française de la Viscose, especially to MM. L. Naudin and J. E. Brandenberger. It is a pleasant duty to recognize technical pioneer work of so high an order.

In addition to these main applications there are many other uses of the reactions involved in the Viscose cycle. Thus "mercerization" of cellulose is an application of the interaction of cellulose (textiles) and caustic soda, to produce lustre effects and finishes upon cotton goods. The structural changes of yarns and cloths determined by these reactions were investigated by John Mercer at a period which long antedates our half century of progress. To Mercer's "genial" anticipations there succeeded a long incubation period. A few discoveries of minor import in this field then revived interest in the major product, the alkali cellulose of the Viscose cycle, and "mercerized goods" became a textile market of first note and importance.

The lecture was illustrated by incidental experimental demonstrations, including the drawing of "artificial" cellulose threads from the cuprammonium solution; also by a selection of specimens representative of the industries specially described. It is proper to mention by name the manufacturing firms who freely supplied these, as they are firms and corporations not merely industrially successful, but pioneers who have developed the technical science of these industries as their essential basis, and the list of names is a technological record.

Cortaulds, Ltd., Coventry. (S. S. Napper, Chief Chemist.)
Artificial silks, monofil and film tissues. Experimental demonstration of artificial thread formation.

Société Française de la Viscose, Paris. (MM. J. E. Brandenberger and E. Defaucamberge.)
"Cellophane" film fabrics. Viscoid solid products.

Messrs. Olive and Partington, Glossop.
Specimens illustrating bi-sulphite wood-pulp process.

Messrs. Tullis, Russell and Co., Markinich, N.B.
Specimens of esparto pulp, papers and by-products.

Messrs. Burgess, Ledward and Co., Manchester. (W. H. Pennington.)
Mercerized yarns, illustrating the technical effects of mercerization. Special fast dyeings of artificial silks.

British Cellulose and Chemical Manufacturing Co., London and Derby. (Drs. C. and H. Dreyfus.)
Cellulose acetate films, varnish and solids. Aeroplane fabrics.

Messrs. J. R. Denison and Co., Bradford. (J. R. Denison.)
Specimens of mercerized cotton fabrics. Special finishes.

The Plaissetty Mantle Co., London. (T. Terrell, K.C.)
Specimens of incandescence gas mantles and stages of special processes.

The Viscose Development Co., Ltd., Bromley, Kent. (O. Mason, Manager.)
Specimens of cap films, illustrating processes of fixing pressure proof caps to bottles, etc.

To Mr. W. MacNab, F.I.C., I was indebted for a collection of Military Service Explosives; and the Director of the Royal Gardens, Kew, through Mr. J. M. Hillier, Keeper of Museums, kindly supplied specimen skeleton leaves.

I was indebted to Dr. J. N. Goldsmith for specimens of celluloid; and to Mr. J. Lawrence, of Messrs. Blair Campbell and Co. (Glasgow), for the preparation of cylinders of wood charcoal demonstrating the volume changes of hard woods in the process of destructive distillation.

[C. F. C.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1917.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

M. Seymore Branfoot,
Mrs. G. Gascoigne,
Mrs. Algernon Mansel,
Mrs. Reginald F. Yorke,

were elected Members.

The Chairman read the following letter, which had been received from the Honorary Member elected at the General Meeting on December 4, 1916 :—

Via in Lucina, 17, Roma.
February 2, 1917.

Your Grace and Much Honoured President,

I have received the Diploma signed by you, notifying my election as an Honorary Member of the Royal Institution of Great Britain.

I am exceedingly grateful at the honour conferred on me, and beg you to offer my best thanks to my new colleagues who unanimously elected me. I appreciated in its full extent this mark of distinction.

Believe me, honoured Sir,
Yours very faithfully,
(Signed) VITO VOLTERRA.

To His Grace the Duke of Northumberland, K.G. P.C. F.R.S.,
President of the Royal Institution of Great Britain, London.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Accademia dei Lincei, Reale, Roma—Rendiconti, Classe di Scienze Fisiche, Mathematiche e Naturali. Serie Quinta, Vol. XXVI., 1° Semestre, Fasc. 1. 8vo. 1917.

American Chemical Society—Journal for Feb. 1917. 8vo.
Journal of Industrial and Engineering Chemistry for Feb. 1917. 8vo.

American Geographical Society—Geographical Review for Jan. 1917. 8vo.

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WEEKLY EVENING MEETING,

Friday, March 9, 1917,

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

COLONEL SIR ALMROTH E. WRIGHT, M.D. C.B. F.R.S. M.R.I.,
A Consultant Physician to the British Army in France.

The Treatment of War Wounds.*

WE are wont to classify the patients in our military hospitals into sick and wounded. In reality all, or nearly all, are suffering from bacterial infections. And in this lies the essential difference between the sick and the wounded, that the sick are suffering from infections spontaneously contracted, the wounded from infections induced by mechanical injuries. My theme is the treatment of this latter class of infections. They are distinguished by certain quite special features.

In spontaneous infection we have to deal with microbes which have fought their way into the body, and generally only a single species of microbe will have done this. We have in wounds microbes mechanically driven in, and every sort of microbe which exists in external nature may thus be introduced.

Furthermore, the disposition of the microbes in war wounds is fundamentally different from that in ordinary surgical wounds. In war wounds the infection is disposed in part upon lacerated raw surfaces; and in part it is carried deep into the substance of the tissues. Moreover, the infected lacerated surfaces are left denuded of skin and exposed to injurious external influences; and the infection which is carried down into the tissues is left as a buried infection. Now there is in ordinary surgical operations nothing at all comparable to this. The surgeon would never leave cut surfaces naked and exposed, or a buried focus of infection unopened.

This will have brought it home to you how especially difficult are the conditions which have to be dealt with in the infected war wound. But let me, before embarking upon the question of their treatment, first tell you something about the natural agencies by which the inroads of microbes are combated. You are, of course, aware that we are guarded against microbic infection by our blood fluids and our white blood corpuscles.

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* [Supplemented by Additional Matter relating to Antiseptics and the Method of Carrel. Reprinted from *Lancet*, June 23, 1917.]

THE BODY FLUIDS.

Let me begin with the blood fluids, and let me take you directly to the following experiment. I call it the experiment of *pyo-sero-culture*—i.e. the experiment in which we implant pus into serum to see which of the microbes of the wound can grow in the blood fluids.

We procure for our experiment a suppurating wound. We take from it a specimen of pus containing a large variety of different organisms. At the same time we take from the patient's finger a sample of blood, and we take a specimen also of our own. When the serum has issued from the clot we take a capillary pipette, fit a rubber teat to the barrel, and inscribe a mark upon the stem at

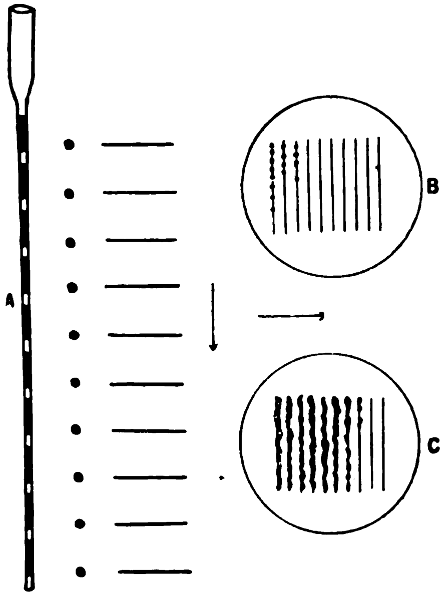

FIG. 1.—*Method of pyo-sero-culture.*—A, Pipette which has been implanted by the wet-wall method, and has then been filled in by the wash and after-wash procedure with unit-volumes of serum. By the side of the pipette to the right is ranged a series of drops representing the series of unit-volumes of serum blown out in order from the pipette, and, finally, to the right of the drops is a series of lines representing linear implantations made upon agar. B, Results of the series of linear implantations made with the unit-volumes of the patient's serum. C, Results of the series of linear implantations made with the unit-volumes of the normal serum which was used as a control.

about say one-third of an inch from the tip. We now aspirate a little pus into the stem, drawing it up only as far as our fiducial mark, and blowing it out again leave a wash of pus upon the walls. This done, we sterilize the tip of the pipette, and then aspirate into the stem a series of unit-volumes of serum, dividing each volume off from the next by a bubble of air. The pipette when filled in this manner presents the appearance shown in Fig. 1; and we have in the proximal end our first and heaviest implantation of pus, and in the distal end our last and lightest implantation. The pipette is now placed in the incubator to allow every microbe which is capable of growing in serum to do so. After an interval of six or more hours we proceed to our examination. What we do is to blow out our series of unit-volumes of serum in separate drops and examine under the microscope; or, better, we plant out a sample of each drop upon a separate seed-bed. Here in B and C you have the results of such culture represented diagrammatically — the meagre crop in B being that obtained with the patient's serum, and the more copious crop in C being that obtained with normal serum.

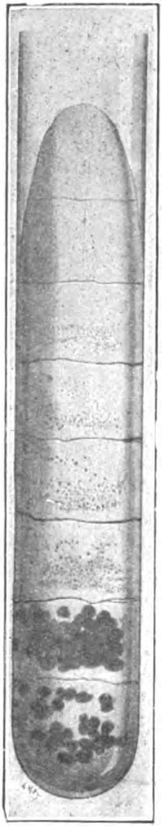

FIG. 2.—A portion of a pyo-sero-culture planted out upon an agar slant divided up by furrows into a series of seed-beds.

And you have in the next figure (Fig. 2) a drawing of an agar tube implanted from a pyo-sero-culture made with the serum of a wounded man. In the upper part of the agar tube you have two seed-plots implanted from the distal portion of the capillary stem. These have remained sterile. In the middle of the tube you see four plots implanted from the unit-volumes of serum which occupied the middle region of the capillary stem. These have grown colonies of only one species of microbe—the streptococcus. At the bottom of the tube you see seed-plots implanted from the proximal end of the capillary stem. These are overgrown with colonies of staphylococcus. But no doubt interspersed with and overgrown by these are also colonies of streptococci. If, instead of cultures from the patient's serum, I had been showing you here cultures from normal serum, what you would have seen would have been a much larger number of fertile seed-plots, and the seed-plots implanted from the proximal end of the pipette would have shown a large assortment of different colonies.

We learn from such experiments three lessons: first, that in the uncorrupted serum in the distal region of the pipette only two species of microbes from the wound can grow and multiply; secondly, that in the corrupted serum in the proximal end of the pipette all the microbes of the wound can grow; and, thirdly, we learn from a comparison of the wounded man's serum with the normal serum that the former offers more resistance to microbic growth, and is less easily corrupted by the addition of pus.

Cause of the Corruption of the Serum.

Experiments of this kind clearly do not tell us the cause of the corruption of the serum. That corruption may be due to some chemical substance contributed by the pus to the serum, or to something special in the character of the bacteria implanted. This point we can clear up as follows. We go back to our very septic wound. We clean it out carefully by syringing. That leaves us with a wound cavity clean but still abundantly infected. We then take the little cupping apparatus which is shown in Fig. 3. We apply it to the walls of the wound, using light pressure. Then, puncturing the attached rubber tube with the needle of a hypodermic syringe, we withdraw the contained air, and leave our lymph leech *in situ* adhering by negative pressure till the time for re-dressing the wound comes round. When we now go back to our wound we find there two quite different discharges. We have in the general cavity of the wound a thick pus containing many broken-down leucocytes and pullulating with all sorts of microbes. In the body of the lymph leech we have a nearly clear lymph containing well-preserved leucocytes and only a very few

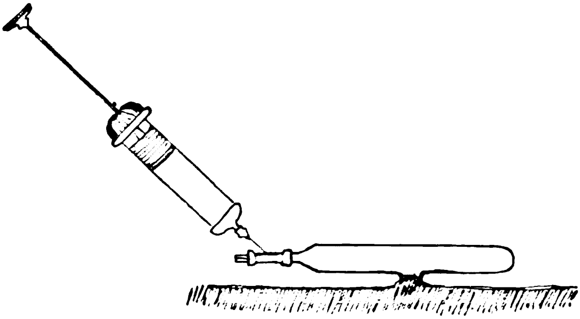

FIG. 3.—Lymph leech in position, showing technique for exhausting the air.

staphylococci and streptococci. Since we had on every part of the walls precisely the same amount and kind of bacterial infection, and since we are in each case dealing with the self-same lymph and leucocytes, this difference of results is imputable, not to our having in the lymph leech a different bacterial implantation, but to the negative pressure having furnished a larger proportion of blood fluids.

But with this the problem is, as you see, only incompletely resolved. We have learned that the corruption of the lymph is not determined by the nature of the bacterial implantation; we have reason to think it is hindered by a larger afflux of lymph; and it looks as if it might have something to do with the breaking down of the leucocytes. But we have not yet put our finger upon the particular element that takes away from the serum its power of inhibiting microbic growth, and converts it into a congenial pabulum for all manner of micro-organisms.

Let me in this connexion invite you to consider, for that may perhaps put us on the path for the solution of our problem, a scheme of classification of the albuminous substances. I would propose to classify them from the point of view of their capacity to furnish pabulum for microbes, and to distinguish three classes of albuminous substances. First, would come *digested albumens*. It is familiar matter that these furnish very congenial pabulum for microbes. In the form of peptone we use them for all our artificial cultures. A second category of albumens would be *native albumens*. Muscle, milk, and eggs furnish such albumens. These are not like digested albumens, directly assimilable. Before they can be assimilated, whether by ourselves or by microbes, they must be broken down into simpler elements by digestion. To that end we, and a certain number of microbes also, are furnished with digestive ferments. There is yet a third class of albumens. I would venture to call these *defended* or *protected albumens*. These cannot, like the digested albumens, be directly assimilated. Nor can they, like the native albumens, be directly digested. They are specially defended against the attack of digestive ferments. The albumens of the serum fall into this class of "defended albumens." It is well known with respect to serum that it has an antizymotic, and in particular an antitryptic, power—a power of neutralizing digestive ferments, and in particular trypsin.* You will, perhaps, not immediately perceive that the fact that the serum is antitryptic in any way elucidates our problem. But let us take that fact and put it in another way and then consider. Let us, instead of saying that the serum has an antitryptic property, say that it has a power of preventing its constituent albumens being converted into pabulum for microbes, and immediately, as I think, light is projected upon our problem. For once we envisage the facts in that way we are immediately impelled to inquire whether the serum's power of inhibiting bacterial growth may not be due to its power of neutralizing digestive ferments, and whether the corruption of the lymph in the cavity of the wound may not be due to a collapse of its defence against proteolytic attack.

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* Let me here incidentally call attention to the fact that the application of horse serum will prevent and very effectually relieve the digestive erosion of the skin which is in enterostomy of the upper part of the small intestine produced by the outflow of pancreatic juice.

That is a point which is very easily settled by direct experiment. And let me now show you what happens when we add trypsin to a serum which has been implanted with microbes. I have here two tubes of a serum implanted two days ago with a minute quantity of pus containing a variety of different microbes. To the one I added trypsin, the quantity added being less than that required to neutralize its antitryptic power. The other tube of the implanted serum served as a control. After implantation both tubes were placed in the incubator. And you see the difference. The trypsinized serum is turbid with microbic growth. That is, we have here exactly the same result as that obtained in our pyo-sero-culture in those volumes of serum which were corrupted by a heavy implantation of pus; and the same result also as was in the lymph leech experiment obtained in the discharges in the wound cavity. Our control serum has, as you see, remained almost perfectly clear. That is exactly the same result as was obtained in our pyo-sero-culture in the distal end of our tube, and again in our lymph leech experiment in the cavity of the lymph leech.

And the doctrine that the antitryptic power is the protector, and trypsin the corrupter, of the blood fluids wins further support from the following facts: 1. In every suppurating wound there is, as we shall presently see, a source from which trypsin can be derived. 2. Blood fluids which inhibit microbic growth are strongly antitryptic; and blood fluids which we find teeming with microbes are tryptic. 3. Examination of the blood shows that all wounded men have a markedly increased antitryptic power, and heavily wounded men on an average a three- or four-fold increased antitryptic power. You saw in our pyo-sero-culture how by that microbic growth is suppressed. You must not miss the meaning of that. It means that the body when endangered takes steps to protect itself non-specifically against all microbic infections of the blood fluids.

THE LEUCOCYTES.

I now pass on to consider the leucocytes and the part they play in the destruction of microbes. You already know with respect to leucocytes that they can emerge from the blood-vessels, burrowing their way out through small pores in the capillary walls; that they make their way to every focus of infection; that they ingest microbes when these have first been prepared by the action of the blood fluids; and, finally, that they can, if things go favourably, digest and dissolve the ingested microbes. There would, by consequence, in connexion with the leucocyte, be three functions to study. First would come emigration, then phagocytosis, and lastly intracellular digestion. Emigration has up to the present been studied only in the interior of the organism. You will realise that means that it has been studied only in a difficult setting and in the presence of all manner of disturbing factors, and you will appreciate that we want now a new and better technique. For we require for the treatment of the infected wound to find out how best to call out the leucocytes; and how, when occasion requires, to restrain their emigration.

I have in connexion with this a technique to describe to you; but first I want you to appreciate what we can and what we cannot expect from leucocytes in the matter of locomotion. Leucocytes can, we know, make their way out through small openings. They can also travel over any ordinary surface. They can edge their way along faster when lightly compressed between two surfaces. They can crawl along strands, creep through a meshwork, and climb a scaffolding. But they are unable to climb a vertical glass wall. And again, they are unable to swim, and so once they get into open fluid they simply go to the bottom. We may liken them to very minute slugs crawling along surfaces and climbing trellises, but brought up short by any considerable barrier of fluid.

All these points must be considered when seeking for a technique for the experimental study of emigration, using for that study specimens of blood withdrawn from the body. The containing blood-vessel can up to a point be imitated by a glass tube, and we can, to facilitate observation, use tubes drawn out flat, such as shown in Fig. 4. But the artificial differs from our natural capillary in having impermeable instead of permeable walls. This, of course, makes emigration through the walls impossible. None the less, these tubes supply what we want for the study of the movements of leucocytes. We can institute races along the length.

But first certain preparations must be made. The course must be cleared of all obstructions—i.e., the red corpuscles must be got out of the way. Next the leucocytes must all be brought back behind the scratch line. Further, we must provide a scaffolding for the leucocytes to climb. All this can be arranged. We fill in our flat emigration tubes with blood and seal them at one end. Then, by centrifuging, we bring the blood fluids to the top and the corpuscles to the bottom. The lighter leucocytes will now have arranged themselves in a layer immediately above the red; and presently the supernatant fluid will clot and the meshwork of fibrin will then provide the scaffolding we require. We can now impose upon the clot—let me for convenience call it the white clot—any chemical agent we please and let it slowly diffuse down to the leucocytes. For the study of the effect of bacterial infection, we can introduce microbes into the blood before this is filled into the tube. Or as an alternative blood can be filled into tubes whose walls have been wetted with a microbic culture. Finally, we set our experiment going by placing our emigration tubes in the incubator—that is, we supply to our leucocytes the necessary warmth. And we can at any moment take stock of what is occurring in our tubes by examining through the walls with the naked eye or with the low power of the microscope. Also by a very simple technique we can extract the clot from the tube and mount and colour it, so as to bring everything clearly into view under the high powers of the microscope.

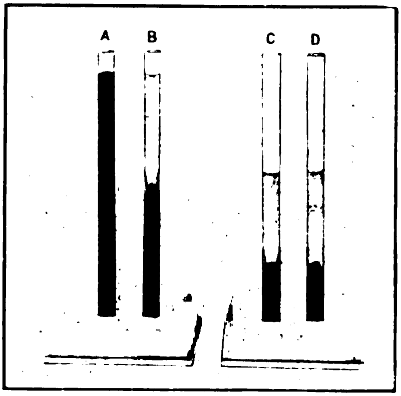


FIG. 4.—Drawing of four flattened capillary tubes—A, filled in with blood; B, a similar tube after centrifugalisation showing above the "white" and below the "red clot"; C and D, similar tubes after incubation. Leucocytic emigration is in each case visible to the naked eye as an opaque white band occupying the lower portion of the white clot. In D, where physiological salt solution had been imposed upon the white clot, the band of emigration is much broader than in C.

Emigration of Leucocytes: Facts with Practical Application.

I must limit myself to showing you in connexion with emigration a few outstanding facts which have a practical application to the treatment of wounds. Let me begin with the naked-eye appearances. We have in Fig. 4, C and D, emigration tubes containing centrifuged blood which have been in the incubator for about eight hours. In C—the control tube—we have centrifuged blood to which no addition has been made. In D some weak salt solution has been imposed upon the white clot. The emigrating leucocytes are visible to the eye in the form of a slightly opaque white band extending upwards from the red into the white clot. You see that in D the corpuscles have climbed higher than in C.

Fig. 5 shows what such a band of emigrating leucocytes looks

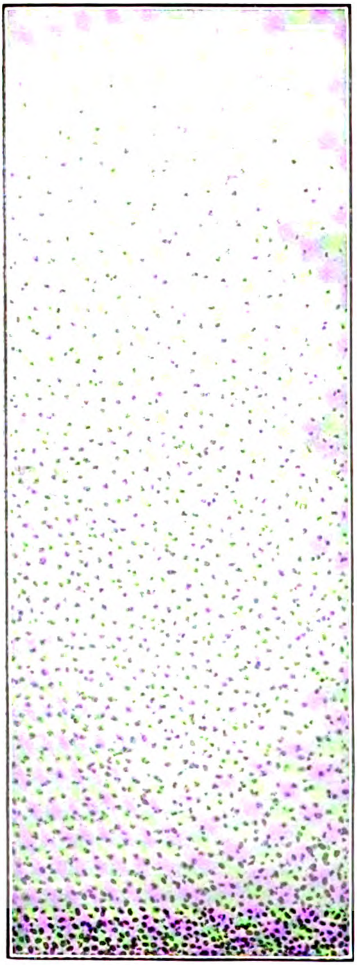


FIG. 5.—Magnified view of the band of leucocytic emigration seen in Fig. 4, D.

like under the microscope. Instead of the leucocytes being all, as you will see in the next figure, congregated together behind the starting line, they here are actively emigrating—the more active outdistancing the others in the race.

Fig. 6 shows what happens when 5 per cent. salt solution is imposed upon the blood. That salt passes down by diffusion and arrests emigration, and I want you to notice on the right of the figure (and more clearly in the inset) that the few white corpuscles which were beginning to emigrate when the salt solution overtook them are broken up and destroyed. By that trypsin will be set free.

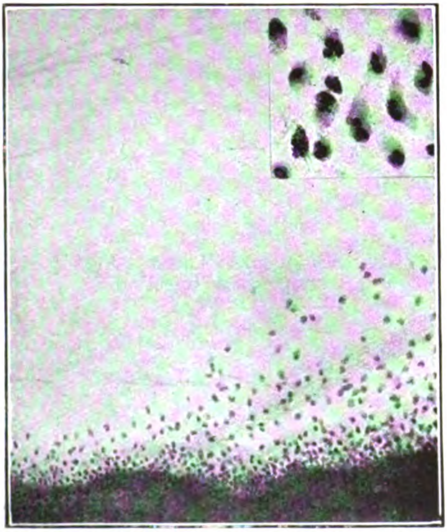


Fig. 6.—Magnified view of the leucocytic layer in the case where strong salt solution was superposed upon the white clot.

I next show you what happens when microbes have been implanted into the blood. Those microbes—supposing always that they are the sort that can proliferate in blood—grow out into colonies. In Fig. 7 is shown what happens when an excessive implantation has been made, and the bacterial colonies come up very thickly in the blood. You see here that emigration is entirely arrested. If that were to happen in infected tissues it would mean that the organism was there giving up the combat against the microbes.

In Fig. 8 we have again streptococcus implanted into the blood, but this time it is a much more sparing implantation. And here, as you see, the leucocytes are carrying out a raid against the microbes, each leucocyte ingesting and filling itself full with microbes.

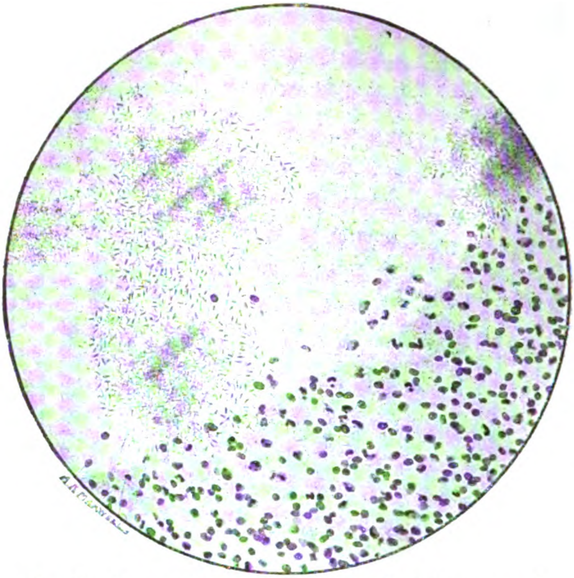


FIG. 7.—Leucocytic emigration restrained by excess of streptococcic infection.

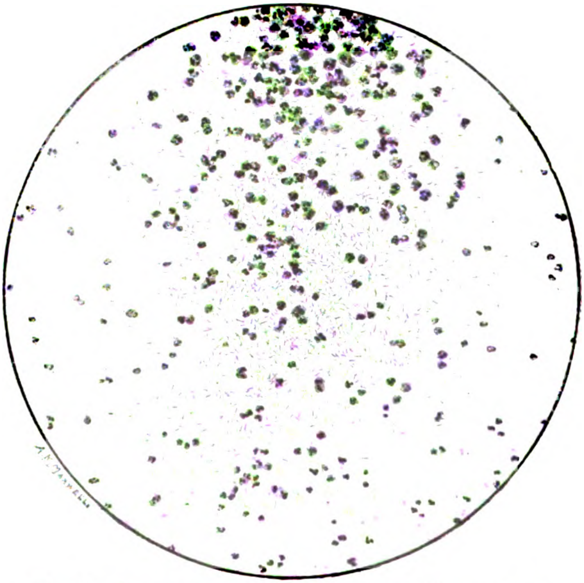


FIG. 8.—Leucocytes emigrating and attacking a colony of streptococci.

In Fig. 9 I show you what happens when we make into the blood a very heavy implantation of the gangrene bacillus. Here in the neighbourhood of the leucocytic layer things are for the moment going well for the leucocytes, for they are actively phagocyting. But farther away from that layer there are very numerous colonies of the gangrene bacillus, which are growing unimpeded. The omens are consequently unfavourable. You can see in your mind's eye what is going to happen. In the first place, all further emigration of leucocytes is going to be arrested. And, in the second place, the leucocytes which have already emigrated and ingested microbes will, instead of successfully digesting them, be gradually poisoned by bacterial toxins. And when the leucocytes are killed, their digestive ferment—trypsin—passes out into the serum. By that, the serum will, as we have seen, be converted into a medium in which microbes can grow and pullulate.

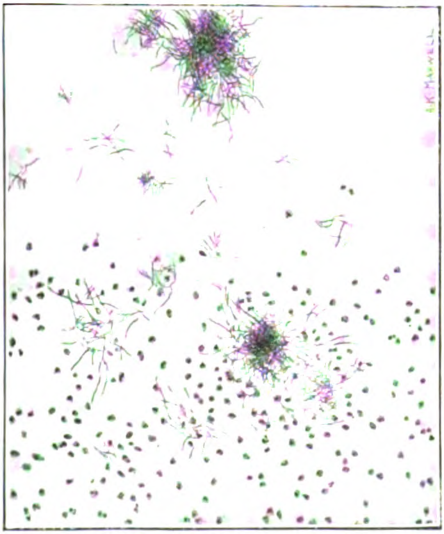
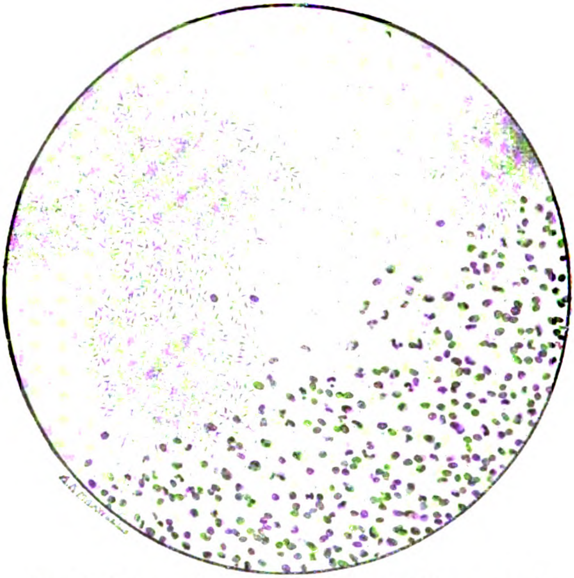


Fig. 9.—Leucocytes emigrating and attacking colonies of the gas gangrene bacillus.

Distinction between "Live" and "Dead" Spaces.

But we must now come back from these general questions to that of the treatment of the wound. Let me begin by explaining to you —for important questions of treatment hinge upon this—the distinction between "*live spaces*" and "*dead spaces*."



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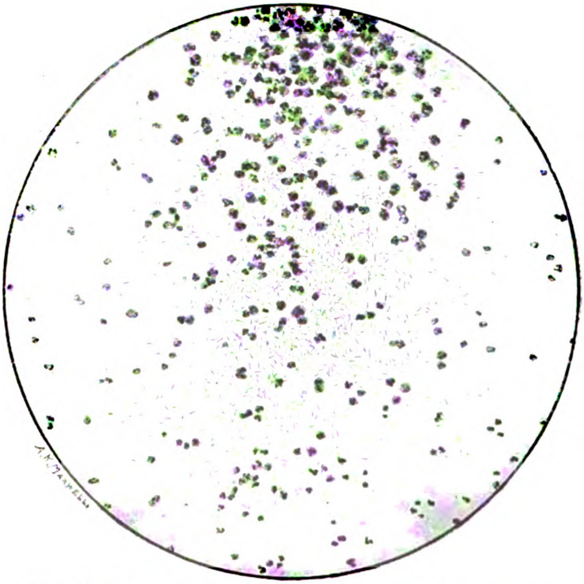

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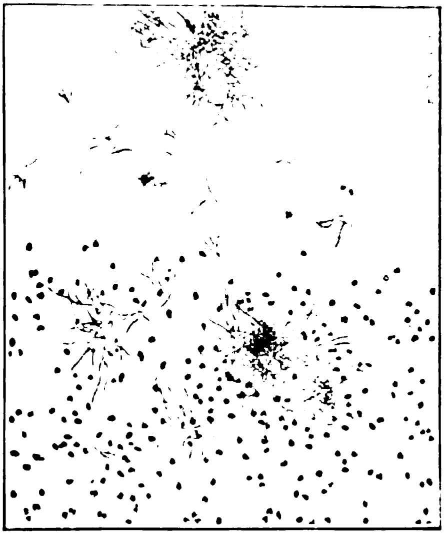

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DISTINCTION BETWEEN "LIVE" AND "DEAD" SPACES.

But we must now come back from these general questions to that of the treatment of the wound. Let me begin by explaining to you —for important questions of treatment hinge upon this—the distinction between "*live spaces*" and "*dead spaces*."

In the lacunæ of vascularised tissues we have *live spaces*. In these we have optimum conditions for resistance to bacterial infection. We have here an antibacterial lymph; and by continuously renewed exudation corruptive changes will be continually antagonised and made good. Again, in live spaces we have terrain that can be effectively searched by leucocytes; and if the bacterial infection should not be extinguished by the first leucocytic attack reinforcements of leucocytes can be supplied from the capillaries feeding the live spaces. In all these respects live and dead spaces are sharply contrasted.

Dead spaces are found in tissues which have been bruised and cut off from their blood-supply, in sloughs, in bone sequestra, and in the texture of cloth and intrusive foreign bodies. And we have a dead space in every abscess sac, under every slough and scab, and also in every collection of pus lying open in the pockets of a wound. The essential characteristics of dead spaces are two. First, they cannot be effectively searched by leucocytes. In some cases chemical, in others mechanical, conditions stand in the way. Secondly, the leucocytes which are engaged cannot be reinforced, nor can the fluid be renewed. Isolation from the circulation makes this impossible.

Let me try to tell you in the fewest possible words what this imports in infection. If in an infected live space the balance turns in favour of the microbes, there is still a chance of its being redressed. At any rate reinforcements can be brought up. In infected dead spaces—and let me here point out to you that all experiments conducted in test-tubes are equivalent to experiments conducted in dead spaces—an advantage gained by the microbes is irretrievable. Counter attack is impossible.

Let me also tell you what happens to the patient when infection flourishes in a dead space. In the first place bacterial poisons will be absorbed, and the patient will suffer from septic fever. In the next place the microbic infection—this will generally be a streptococcic infection, but in war wounds it may be a gangrene-bacillus infection—will very often invade the surrounding tissues and from thence the blood-stream. And in the third place, the pus in the dead space will, when it becomes tryptic, eat its way into the enclosing tissues. The containing sac will then enlarge in all dimensions, the pus in the case of an abscess burrowing in the direction of least resistance until it opens upon an inner or an outer surface.

TREATMENT OF WOUND INFECTIONS.

If you have now quite clearly apprehended the significant distinction between a live space and a dead space, you will with that have mastered the first great principle governing the treatment of all local bacterial infections. If you are dealing with infection in live spaces you can often mend matters by bringing (that is the rationale of hot fomentations) a larger blood supply—that means more lymph and more leucocytes—to the focus of infection; and again you can often mend matters by improving the quality of the lymph--that is the rationale of vaccine therapy; or, again, you may apply both these procedures concurrently. But when you are dealing with an infected dead space you cannot mend matters by these procedures. You might just as well apply them to a test-tubeful of infected fluid. Where you have infected dead spaces, your remedial agent is the knife. You have to evacuate your dead space as I empty this test-tube.

Now that is the whole purpose and meaning of the surgery of the wound as carried out at the front. You have there two guiding principles. Every infected dead space must be cut down upon and evacuated. And, as a prophylactic measure, every space which would, if left to itself, become an infected dead space—that means every space occupied by an infected projectile or pieces of infected clothing or infected foreign bodies or devitalised infected tissues—must likewise be laid open and cleaned out. That completes the chapter that relates to buried infections.

But it is only the beginning of the treatment of the wound. There is still the surface infection. The situation you have to face is just the same as that produced by emptying an infected tube: you have got rid of the infected contents, you have left the infection on the walls.

I now turn to the problem as to how best to deal with this infection. And here again inevitably we must establish distinctions. We must distinguish between the *naked tissue surface* made by the act of the projectile, or section with a knife, and the *granulating defensive surface*, which after a time clothes the naked tissues. In the former we have a non-vascularised surface, and in this a system of lymph spaces left without mechanical or biological protection other than that furnished by the emigration of leucocytes and (till that stanches) by the outflow of lymph. And the naked tissue surface is not only ill defended against microbic attack, it is also peculiarly liable to damage and to physiological deterioration of the kind which opens the door wider to such an attack. Such a surface readily dries up; and drying means the closing down of the capillary circulation. Again, a naked tissue surface, seeing that it is non-vascularised, readily takes cold, and by that both lymph outflow and emigration are arrested. And, lastly, a naked tissue surface, if kept wet, will, so soon as the discharges become tryptic, readily undergo erosive digestion. Against all these forms of physiological degeneration special provision should always be made.

A granulating surface offers much greater protection against microbic infection, and is much less subject to damage. The tissues are covered in by many layers of protective cells, the lymph spaces are sealed over, and there has been laid down immediately below the surface in newly formed vessels a very abundant blood supply. All this is protection against massive microbic invasion from the surface: against the wound taking cold; and against erosive digestion. In short, there is with an infected granulating surface much less danger of a set-back than with an infected naked tissue surface.

The Natural History of the Wound with a Naked Tissue Surface Left to Itself.

Let us consider the natural history of the untreated wound with an infected naked tissue surface. I will take the case of an open shell wound left without treatment. According as it is wet or dry the evolution of this wound will be entirely different. Let us suppose that it is allowed to dry. Under the original dry dressing the blood and lymph flow from the surface will gradually stanch, and we shall then have a naked tissue surface with a coating of coagulated blood and lymph. In this will be incorporated elements of moribund tissue, other elements of foreign matter, and always a certain number of microbes. Little by little the coating of coagulated blood and lymph upon the surface of the original wound, or of the surgeon's incisions, will dry up, and by that the capillary circulation will be closed down. And all the while the serophytic microbes will be proliferating. As a result of all this the superficial tissues will die, and become gangrenous, and the originally clean naked tissue-surface will gradually be transformed into a dry, greenish-black, excessively fœtid, slough-covered surface pullulating with microbic growth.* Under the sloughs will then be formed infected dead spaces, and from these the infection—I am here thinking in particular of a gangrene infection-- will invade the neighbouring live spaces, converting these in their turn into dead spaces until we have to cope with large areas of gangrene and a general intoxication.

That, of course, will happen only with very heavy infection or extreme physiological deterioration. With lighter infection or less adverse physiological conditions the invaded organism will have recourse to measures of defence. Gradually the superficial sloughs and gangrenous portions of the deeper tissues will be demarcated and then amputated from the living tissues—the amputating agent being, no doubt, the tryptic ferment in the dead spaces. And at the same time there will have been organised in the living tissues some little way back a defensive front built up on the same plan as a granulating surface.

Let me now tell you also what will happen if the infected surface is simply kept wet. Here, also, the microbes which have been incorporated in the clot would grow out. Then there would supervene leucocytic emigration, and upon that would follow a breaking-down of the leucocytes with a setting-free of trypsin ; and after that any and every microbe would pullulate in the cavity of the wound and on the devitalised wound surface. Finally, if treatment were still deferred there would be reproduced in an aggravated form (for there would in the open wound be a varied and more formidable infection) the evil train of events which is associated with infection in a buried dead space. When you reflect that an open wound cavity filled with tryptic pus is physiologically equivalent to an unopened abscess sac, you will see that erosive action will enlarge and deepen its cavity ; that this will enable the microbic infection to burrow everywhere deeper into the walls ; and that bacterial poisons will be absorbed

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* Let us note in connexion with this that the albuminous substances of our tissues, when no longer bathed in lymph, are immediately degraded to the rank of unprotected native albumens.

All I have been saying in the last few minutes can be compressed into this—An infected naked tissue surface becomes, if allowed to dry, *a desiccated slough-covered wound* ; if simply kept wet, *a tryptic suppurating wound.* And the bacteriological events can also be expressed in a single sentence. A comparatively light infection, such as we have in the man whose wounds have been properly opened up and mechanically cleaned, is converted into a very heavy infection ; and a purely surface infection into an infection invading the deeper tissues.

PROBLEM OF PREVENTING THE DEGENERATION OF THE WOUND WHEN TREATMENT IS INTERRUPTED DURING TRANSPORT.

Having realised what happens to the wound when untreated, we have to think out how to keep wounds—whether originally completely open or opened by a surgeon – from falling during protracted journeys from hospital to hospital into these desperately unwholesome conditions. We have also to consider how to restore, as rapidly as possible, wounds which have lapsed into distressing conditions either through lying out untreated on the field or through interruption of treatment during lengthy transport.

Suggestion that the wound could be sterilised at the outset, and could be kept sterile by leaving an antiseptic in the wound.—The first thought of every man would probably be that the wound should be most carefully disinfected at the outset. But what happens in burns shows that to start in open wounds with a sterile surface avails nothing. A burn is at the outset absolutely sterile, and quite notoriously—no doubt the germs begin to arrive before the burnt surface has well cooled off –it tends to become very rapidly intractably septic. We may take it that the emigrating leucocytes are held back in the superficial sloughs, disintegrate there, and corrupt the exuding lymph. And this cannot be prevented by any application of antiseptics. It is just the same with war wounds. These become heavily infected even when they are drenched at the outset with undiluted carbolic acid or concentrated solutions of iodine.

This is not the place for any lengthy discussion of the reasons for this failure of antiseptics. But the gist of the matter can be put quite shortly. The current belief in the therapeutic efficacy of antiseptics rests on experiments which are quite fallacious. They are fallacious in that in them the antiseptic was applied in watery media—media which left that antiseptic unaffected. To have value —that is, to have application to conditions obtaining *in vivo*— the experiments should have been conducted in pus or serum —media which quench antiseptic action. Again, in the experiments of the past the antiseptics were intimately mixed with the bacterial suspensions; whereas, applied in the wound the antiseptic comes only into external contact with the infected wall, and the inflowing discharges. Employed thus we cannot expect it to diffuse into and exert a bactericidal effect either in the infected wall or in the discharges.

By reason of these considerations having been disregarded, the issue as to whether antiseptics applied in the wound with prophylactic intent can be of any use must be investigated *de novo*.

Experimental Investigation of the Efficacy of Antiseptics.

Let me now try to indicate to you what sort of experiments should be undertaken before nourishing in connexion with a particular antiseptic, the expectation that it is going to be efficacious for sterilising and afterwards suppressing microbic growth in wounds. I can illustrate my points best if you let me show you here four tubes.

In Tube No. 1 I have a suspension of microbes in water. I now add an equal volume of the antiseptic I wish to test, and shake up thoroughly. These are, as you see, conditions which give every possible advantage to the antiseptic. It is applied in a non-albuminous medium, and is intimately mixed with the microbes. To find out whether the microbes have been killed I draw off a sample and dilute with very many times its volume of nutrient medium. I then incubate to see whether I get any bacterial growth.

In Tube 2 I make the conditions more favourable to the survival of the microbes—infinitely more favourable than if I left behind an antiseptic in a wound. I have here a mixture of staphylococci, streptococci, and gas-gangrene bacilli suspended in serum, and I now, as in Tube 1, add an equal bulk of the antiseptic and shake up, and I then, following the technique of Professor Beattie, pour on a little hot vaseline which will afterwards congeal. This, forming an air-tight seal, will allow the gangrene bacillus, if it survives, to grow out. It will also announce the growth of this microbe, for it will confine any gas which may be evolved from the culture.

Tube 3 is, as you see (Fig. 10), a tube which has been drawn out into a number of hollow spikes to imitate the diverticula of the wound. My colleague, Dr. Alexander Fleming, its author and inventor, calls this form of tube the "artificial war wound." To imitate the conditions obtaining in the actual war wound we fill both the tube and its diverticula with an infected trypsinised serum. We now empty the tube, leaving behind of necessity in the diverticula a certain amount of the original infected fluid. We then fill with an antiseptic; and the future of the infection will now depend on the penetrating power of the antiseptic. If the antiseptic penetrates into the infected fluid sterilisation will be obtained; if it fails to penetrate, microbes will survive. To test our result we empty out the antiseptic, refill with trypsinised serum, and incubate.

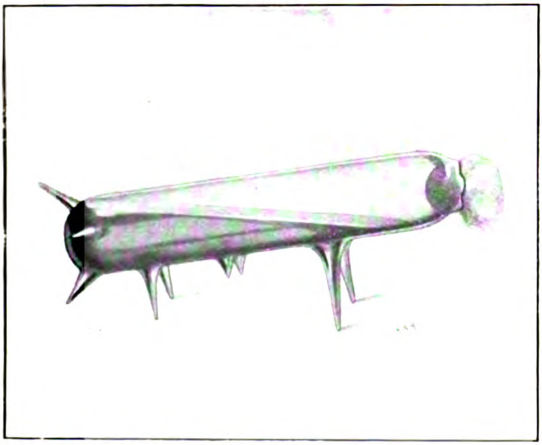

FIG. 10.—A test-tube standing on spike legs, representing a war wound with diverticula.

After asking in Tube 3 whether the antiseptic can completely sterilise a wound which has its recesses filled with an infected albuminous fluid, I go on in Tube 4 to investigate the question as to how far the antiseptic can penetrate into the walls of the wound. *Tube* 4 is, as you see, a tube with hollow spikes. I have coated the inside with infected serum agar, and the spikes provide in their hollows a greater depth of infected lining and also a securer purchase for the serum agar. The prepared tube is filled with antiseptic. We then after an interval pour out the antiseptic, fill in with nutrient broth or trypsinised serum, and incubate. Any microbes which have been left alive in the lining will now grow out into colonies which can be inspected through the walls of the tube.

Let me show you a set of typical results obtained by the test-procedures just described, using Dakin's antiseptic, to-day perhaps the most popular of all antiseptics.

In Tube 1 we have obtained, as you can see by these sub-cultures, complete sterilisation. And it was obtained after only momentary contact with the antiseptic. In Tube 2, where a lightly infected serum was shaken up with an equal bulk of the antiseptic and then incubated, we have in our mixture of serum and antiseptic a very vigorous growth of microbes. You see the medium has become turbid, and there has been an evolution of gas which has pushed up the plug of congealed vaseline. In Tube 3—and here the antiseptic stood for four hours in the tube—we have in the barrel a teeming multitude of microbes. And in Tube 4, after four hours' contact with the antiseptic, only that very thin layer of the infected lining which coats the barrel has been sterilised, in the depth of every spike the bacterial colonies have come up quite thickly, and only in immediate contact with the antiseptic have the microbes been killed. I here show you also in a companion tube which has been incubated 24 hours longer that the microbes you have seen growing in the deeper layers very soon penetrate the sterilised superficial layer, and grow out in the culture medium in the barrel of the tube.

When we find an antiseptic giving results quite different from those here displayed it will then, for the first time, become a rational programme of operations to use, and leave behind, an antiseptic in a wound with a view to safeguarding the patient during lengthy transport.

Suggestion that the bacterial infection in the wound can be kept down during transport by frequent re-applications of an antiseptic.—In the earlier period of the war the only method of re-applying an antiseptic was that of taking down the dressing, syringing the wound, and completely re-dressing. That was, especially in the case of deep wounds and compound fractures, a very lengthy and painful procedure, and one which was nearly impracticable in transport. For that complete re-dressing there has now been substituted by Carrel a procedure for washing and refreshing the surface of the wound through rubber tubes. According to Carrel, Dakin's antiseptic should be employed, and this should be applied every two hours. About the application of this in transport let me say this: that it would, I think, be impracticable to carry it out on a sufficiently large scale and sufficiently systematically; and Dakin's antiseptic applied in an unsystematic manner gives exactly the same results as simply keeping the wound wet.

Suggestion that the set-back in the wound during transport could be prevented by dressing with hypertonic salt solution.—The set-back in the wound with its resulting tragedies could, I think, be avoided by drawing out lymph in a continuous manner from the tissues, and holding up the emigration of leucocytes. The outflow of lymph would drive back and expel invading microbes. It would also prevent the conditions in the walls of the wound becoming unwholesome to leucocytes. The continuous outpouring of lymph would also effectively combat the corruption of the discharges in the cavity of the wound. And lastly it would prevent any drying up of the wound. The effect of holding up the emigration of leucocytes would be to prevent the corruption of the wound discharges. You will remember that leucocytes, breaking down, furnish the trypsin which corrupts the discharges.

We have in a hypertonic solution the therapeutic agent we require for these purposes. The proper way of using it is to apply to the wound three or four layers of lint thoroughly soaked in 5 per cent. salt solution: to impose upon these, as a reinforcement, three or four more layers of lint, thoroughly soaked in saturated salt solution,* and then cover the whole with jaconet, or other impermeable material.

REMEDIAL TREATMENT.

I now pass from discussion of the method of preventing the set-back that occurs in transport to the discussion of its remedial treatment. The set-back will, as we have seen, have given us either a tryptic suppurating wound or a dry slough-covered wound. In each case the *first item in treatment* will be to get a clean surface. For that it will, in the case of the tryptic suppurating wound, suffice to wash away the tryptic pus. In the case of the desiccated slough-covered wound we must get rid of the sloughs. The rational way to do that will be by *cleansing digestion.* Such cleansing digestion can be obtained by treating the wound with hypertonic salt solution. This will, as we have already seen, break down leucocytes, setting free trypsin, and then the free trypsin will rapidly, and especially rapidly if hypertonic salt becomes diluted, amputate the dead from the living tissues. Let us note that what we set out to do by the use of hypertonic salt solution is only to achieve more rapidly, and, as we shall see, with less risk of infection, what putrefaction and the destruction of leucocytes by microbes would, if we allowed things to run their course, spontaneously accomplish. The *second item* of treatment in each case will be to combat the infection which has found a lodgment in the walls of the wound cavity. To deal with this we require an outpouring lymph stream, obtained by hypertonic salt solution.

If the train of reasoning I have laid before you is correct, it will follow that hypertonic salt solution is the agent we require both for preventing the set-back due to interruption of treatment in transport, and also for remedial treatment.

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* The saturated solution diluted with 6 parts of water will give us our 5 per cent. salt.

EXPERIMENTS WHICH EXHIBIT THE PROPERTIES OF HYPERTONIC SALT SOLUTION.

You will very reasonably here expect me to produce experiments to show that a hypertonic salt solution has the virtues I ascribe to it. You will wish to see for yourselves that it attracts water, draws out fluid from moist tissues, sets free trypsin from pus, and initiates digestion.

Drawing Action of Strong Salt Solution.

I have here (*a*) agar containing salt to saturation and (*b*) plain water agar—cast into cylinders in similar 100 c.c. measures, and rendered insoluble by an addition of formalin. Let us consider first the cylinders of water agar. Upon the one of these as it stood in the measure was imposed saturated salt solution, solid salt being afterwards supplied as required. You see that the salt has here drawn out water copiously. The agar has everywhere detached itself from the walls, and we have in the interspace and on the top of the cylinder some 40 to 50 c.c. of fluid. The control cylinder of water agar which has stood in the measure has, as you see, not shrunk at all.

Exactly the converse has happened with the cylinders of salt agar. These were extracted from the cylinder measures: one was put down to steep in water—that one has, as you see, swollen to twice its bulk; the other was immersed in saturated salt solution—that one has not altered in bulk.

We thus see that salt attracts to itself water. It can, according as it is outside or inside, draw it out from or draw it into a moist menstruum.

I have here in these three test-tubes another form of moist menstruum—cotton wool impregnated with egg-albumen. The plug—in each case a very tight plug—is, as you see, placed half way down the tube, dividing it into an upper and lower compartment; and immediately below the plug I have a lateral hole (now sealed with plasticine).* Through this hole I can wash out both compartments very thoroughly, and fill and empty them without disturbing the plug. I have used in these experiments three fluids: a diluted egg-albumen with a specific gravity of 1026 for my plug, and two extracting fluids—water, and a saline solution containing about 8 per cent. of common salt with a specific gravity of 1052. In *Tube* 1 I have water above and below; in *Tube* 2 salt solution above and below; and in *Tube* 3 water above and salt solution below. And here are the results obtained 24 hours after by emptying out the fluids and boiling them *secundum artem.* In *Tube* 1 we have in the fluid from the upper compartment a mere trace of albumen ; in that from the lower a very massive floccular deposit. That means that in the upper only a mere trace of albumen has diffused upwards against gravity, and that in the lower compartment the heavier albuminous fluid has sunk to the bottom. In *Tube* 2, where both compartments were filled with salt solution, we have in the upper a massive floccular deposit, and in the lower a heavy, but not nearly as heavy a one. It is the result in the lower compartment which specially interests us. It shows that an albuminous fluid is drawn down into salt solution in opposition to gravity. In the upper compartment we have, superadded to the drawing action of the salt, a displacement upwards of a lighter by a heavier fluid. In *Tube* 3

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* In sealing with the plasticine care must be taken not to produce a positive pressure, and so drive albumen up into the upper compartment.

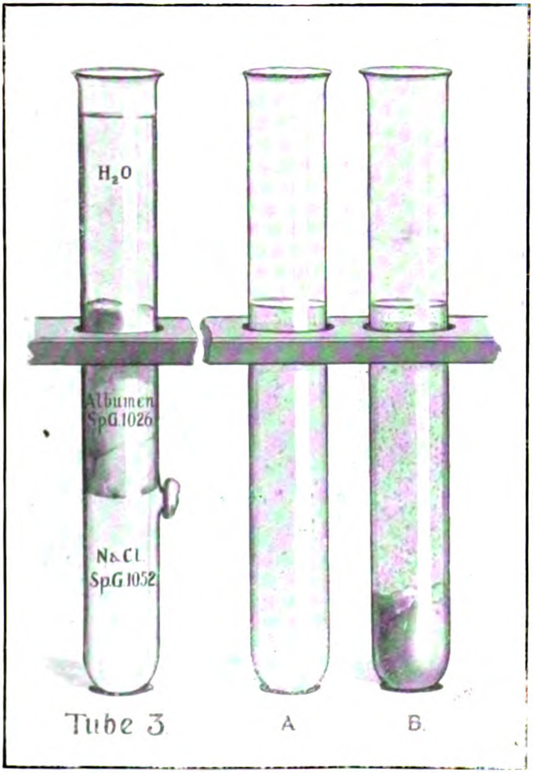

FIG. 11.—Tube 3 is flanked with test-tubes labelled A and B showing the result of boiling the fluid from the upper (A) and the lower (B) compartments of this tube.

(see Fig. 11), with water above and salt solution below, the upper compartment contains, as in Tube 1, only a mere trace of albumen. In the lower compartment we have again, as in Tube 2, a very heavy deposit of albumen drawn down against gravity.

I have here other experiments of which I show you photographed drawings. All these bring out that when strong salt solution and water are brought into opposition, and we introduce such a succession of obstacles as are provided by a cotton-wool plug or a wick, we get, so far as the convection of fluids is concerned, the same results as when we interpose a thin membrane, such as parchment paper or a film of formalin gelatine. With respect to the character of the fluid there is marked difference. Where we employ macroscopic pores albuminous substances are carried through; where we employ only very minute pores we filter out albuminous substances.

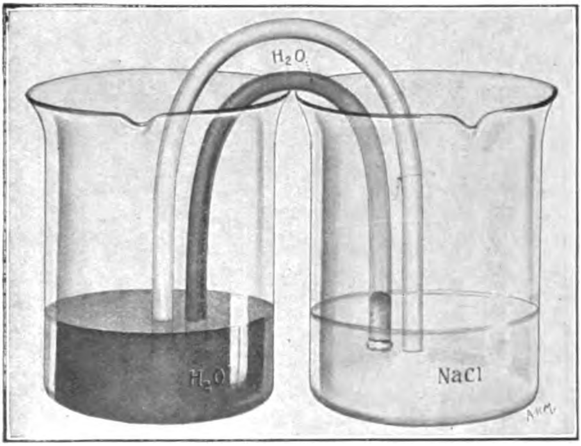

FIG. 12.—The left-hand beaker contains water coloured with methylene-blue; the right-hand beaker strong salt solution. The beakers are connected up by two siphon U tubes—the upper being an open siphon filled with uncoloured water; the lower a siphon plugged with cotton-wool and, like the upper siphon, filled in at the outset with uncoloured water.

In my next experiment (Fig. 12) we have in one beaker saturated salt solution and in the other water coloured with methylene-blue. To maintain the fluids at the same level we connect them up with a siphon filled with uncoloured water. We then establish a further connexion by means of a siphon filled with uncoloured water plugged at the one end with cotton wool. This end goes down into the salt solution, but it is only just immersed. When we now watch events—

and it is a question of watching them for weeks—we find that the coloured water is drawn up into the open limb of the plugged siphon in the manner shown. In the case of the open siphon nothing of this sort occurs.

In the next experiment (Fig. 13) we have a series of three tubes arranged tandem and connected up by wicks of moist bandage fitting quite loosely in siphon tubes. We have in Tube A plain uncoloured water, in Tube B water coloured with methylene-blue, and in Tube C saturated salt solution—all these being filled in to exactly the same height, and then protected against evaporation. We now observe that the level of the fluid in C gradually rises, while that in A and B

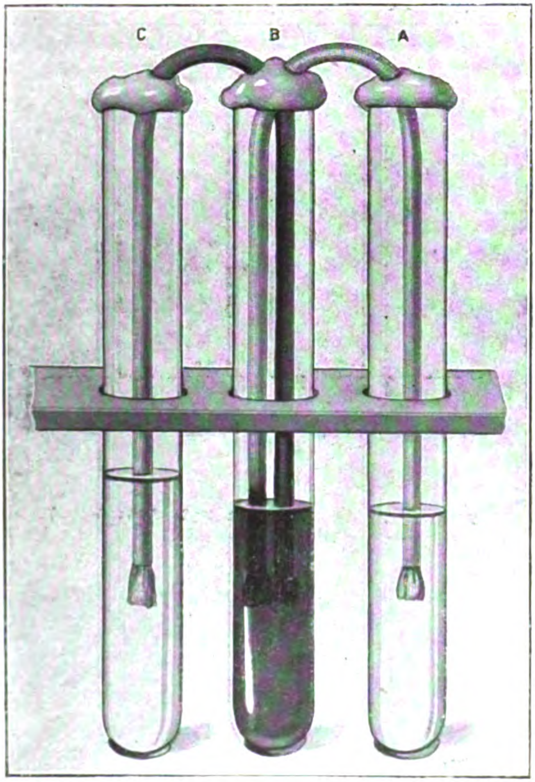

Fig. 13.—Tube A contains uncoloured water; Tube B, water coloured with methylene-blue; and Tube C, saturated salt solution. The fluids stood originally at the same height in each tube.

sinks; and at the same time the colouring matter from B is gradually conveyed up the wick in the direction of C, but is not carried up in the direction of A.

In the next experiment (Fig. 14) we have again a series of three tubes arranged tandem and connected up by wicks as in the last experiment. In A we have plain water, in B an albuminous fluid, and in C saturated salt solution—all filled in to exactly the same height. As in the last experiment, the level of the fluid rises slowly in C and sinks away in A and B. We then unship Tubes A and C, add salt to A, and then boil both. We find that a great deal of albumen has been carried over into C, and that none has diffused over into A.

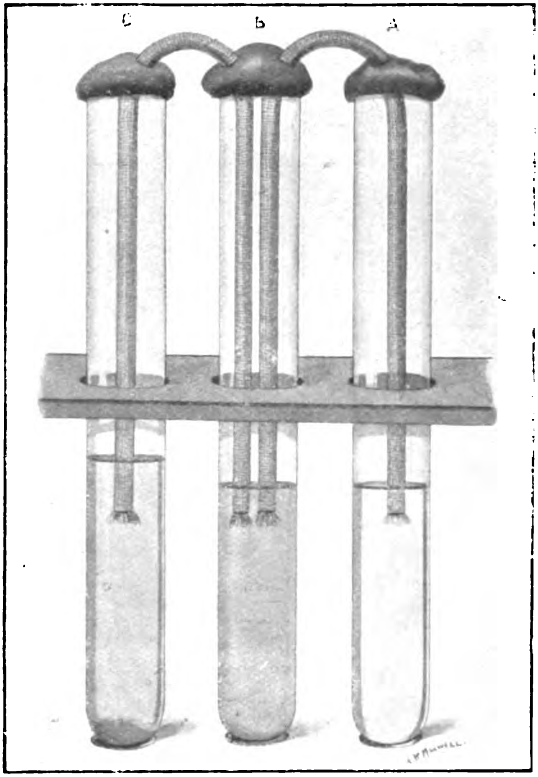

Fig. 14.—Tube A contains plain water, Tube B a watery solution of egg-albumen, and Tube C saturated salt solution.

The Setting Free of Trypsin by Hypertonic Salt Solution.

You will probably not wish to see any further experiments on this point. But I have still to make good to you that a hypertonic salt solution sets free trypsin from pus and initiates digestion. On that matter I will content myself with showing you the two following experiments.

Experiment 1.—I have here, as you see, two test-tubes filled nearly to the top with egg-albumen. To this was added ½ per cent. of carbolic acid, and the albumen was then solidified by immersing the tubes in boiling water. That done I took two cotton-wool plugs and steeped them in a pus to which I had added ½ per cent. carbolic acid. I then inverted my tubes, the one into a beaker containing 5 per cent. salt, the other into a beaker containing physiological salt solution. (Fig. 15, A and B). To these also I added ½ per cent. of carbolic acid. You will understand why I chose carbolic acid as my antiseptic when I tell you that it is an antiseptic which does not destroy trypsin or impede digestion. You see in the drawings made after the tube had been incubated for 48 hours, that in Tube A (the

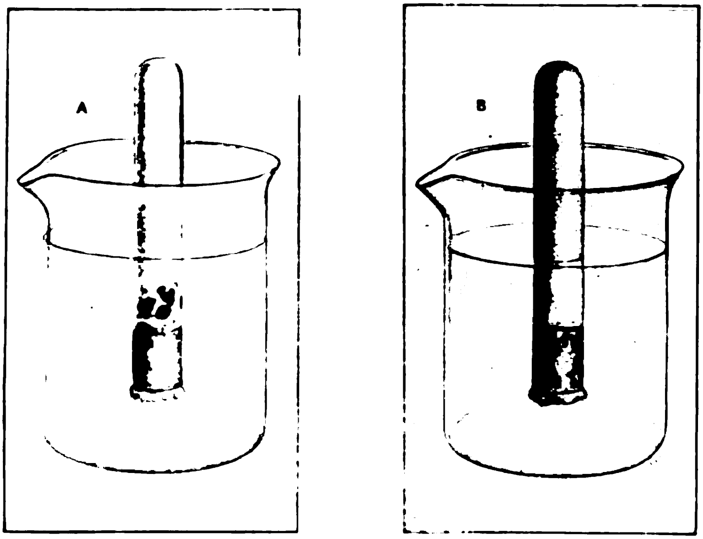

FIG. 15.—Test-tubes filled with coagulated egg-albumen; then plugged with cotton-wool impregnated with pus; and then inverted into beakers. Beaker A contains hypertonic, Beaker B normal salt solution.

tube which was immersed in hypertonic salt solution) the egg-albumen was extensively digested, while in Tube B there was only a mere trace of digestion.

Experiment 2.—I here try to imitate the conditions of slough-covered wounds. I have in these beakers a foundation of coagulated white of egg containing 0·5 per cent. of carbolic acid. On the top of this I have in each case a disc of lint, woolly side up, firmly fastened down by adding another layer of egg-albumen and coagulating this by heat. Upon the lint I have poured a non-tryptic pus, giving, of course, an equal amount to each beaker. In this way I have made what I think can pass as a fairly close representation of a pus-impregnated slough firmly adherent to the floor of a wound. (Fig. 16, A and B.)

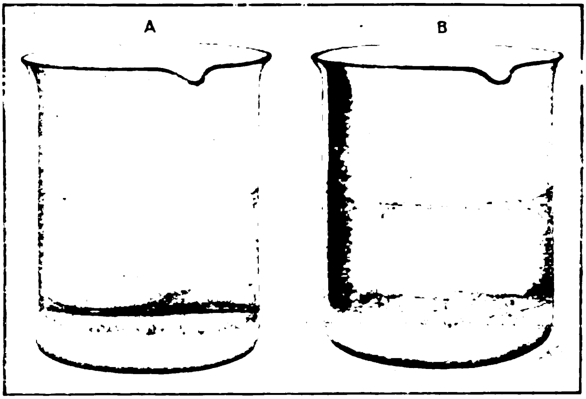

Fig. 16.—Beakers containing coagulated egg-albumen into which is imbedded a layer of lint. Upon the lint was poured pus, and upon this in the case of Beaker A hypertonic, and in the case of Beaker B normal salt solution. In Beaker A the artificial slough has separated off by tryptic digestion.

We now pour upon one of the artificial sloughs 5 per cent.; upon another 0·85 per cent. solution made up with ½ per cent. carbolic acid; and we may pour upon a third Dakin's solution. We now place them all in the incubator. You see here what has happened after 24 hours. In *Beaker A*, where the artificial slough has been treated with hypertonic salt solution, the slough has loosened itself from its bed, and floats up as I pour in water. In *Beaker B*, where I imposed only physiological salt solution, the slough is still firmly adherent. And the same holds of *Beaker C* (not figured), where we have Dakin's solution.

We have here, as you see, an instructive experiment. And we can derive from it more instruction than has as yet appeared. In the first place, if we carry the experiment further, the slough which has been treated with physiological salt solution will also separate. In contrast with this will be what will happen with the artificial slough treated with Dakin's solution. Here, especially if the Dakin's solution is periodically renewed, the clot will remain adherent. For the Dakin's solution, while it is destructive to leucocytes, is, when not quenched by albumen, destructive also to the trypsin which brings about the separation of the slough.

We can always appreciate the situation and tell what is going to happen whether in the wound or *in vitro* by taking samples of the fluid and testing quantitatively for trypsin with milk containing about ¼ per cent. of calcium chloride cryst., and we can when making experiments *in vitro* on the separation of artificial sloughs, hurry up the events by conducting the digestions at 50° C.

We can also vary our experiment by leaving out the antiseptic. Then microbic growth will, more particularly in the weaker salt solution, proceed unchecked, and the destruction of leucocytes by that growth will play an important rôle in the loosening of the clot. Meanwhile, the microbes will be everywhere making their way deeply into the albuminous substratum. This imitates what is occurring in every untreated slough-covered wound. While on the surface sloughs are decomposing and separating, in the depth further tissue is becoming heavily infected and gangrenous. Since we cannot block the infection by antiseptics, we must place mechanical and biological obstacles in its path. That means we must get lymph to pour out in full stream from the deeper tissues, and attract leucocytes into those tissues. We may then, after the sloughs have separated, look to have a clean, comparatively lightly infected wound surface.

TREATMENT OF THE WOUND IN THE CASE WHERE WE HAVE ONLY A SURFACE INFECTION.

When we have got back to a clean and only lightly infected surface we must think out our next step. It will help if we first review what we have learned and get things into proper perspective.

We have learned that there are in wound infections two supreme dangers. *First*, there is the danger associated with the buried infection. We have appreciated that the effective and only remedy for this is the immediate opening up of the infected dead spaces. That, you will remember, is a question of converting a buried infection into a surface infection. The *second* very serious danger is that intensification of the surface infection which follows upon a lengthy interruption of treatment during transport. This, regarded from the point of view of loss of life and limb, ranks next in order of importance after delay in dealing with the buried infection. When the set-back due to transport has been prevented or remedied, we have confronting us the problem which, if treatment had been uninterrupted, would have presented itself earlier—the problem as to how to treat a slight infection of a naked tissue surface.

One procedure is to leave the wound to heal up from the bottom, limiting oneself to such re-dressing as would prevent erosive digestion. By this programme the patient would, when his wound is a large one, be condemned to very many months of disability and also of bacterial intoxication. For the fact has got to be faced that it is all but impossible to maintain satisfactory conditions in a large wound for months on end.

The alternative programme is for the surgeon to close the wound with the minimum delay. If the anatomical conditions permit, and the bacteriological examination shows the wound surface to be practically uninfected, or if the wound is only a very few hours old and the implanted microbes cannot yet have grown out, the wound can, after removal of all dead and foreign matter, be immediately closed—the surgeon, of course, standing by to reopen the wound if symptoms of buried infection develop. If, on the other hand, bacteriological examination shows that the wound surface is appreciably infected, or the history of the case makes this practically certain, we should, by closing the wound, be violating all the principles of surgery. We should be converting a surface infection into a buried infection. The proper step to take with a wound which is appreciably infected is to reduce the microbic infection to the point at which it is negligible and then re-suture.

Methods of Dealing with a Microbic Infection which Stands in the Way of Secondary Suture.

The microbic infection may be dealt with by any one of the following procedures.

In the *first place* we can employ the physiological procedure. If we elect to do this, we think out clearly the requirements. For example, it will be inappropriate when dealing with a purely superficial streptococcic and staphylococcic infection to continue the application of hypertonic salt solution. The effect of that would be, on the one hand, to hold off phagocytes from the microbes (for strong salt arrests emigration); and, on the other hand, to provide the staphylococcus and streptococcus with lymph, a fluid in which they can grow and disseminate themselves over the whole face of the wound. What we want is an application which calls out leucocytes, and which will restrain, or which at any rate will not activate, the lymph flow. Physiological salt solution, and zinc sulphate in $\frac{1}{4}$ per cent. solution, and no doubt many other heavy metal salts in dilute solution, are the sort of agents we require. But what is, above all, essential to success in physiological treatment of a surface infection is assiduity in removing any leucocytes which may break down upon the face of the wound. That is a question of maintaining intact the antitryptic power of the lymph on the wound surface.

A *second method* of procedure—I may call it the *unreasoning antiseptic procedure*—is to employ an antiseptic, without laying stress upon the assiduous cleansing of the wound surface and the maintenance of good physiological conditions; without inquiring whether the antiseptic can, when brought into external contact with pus or an infected tissue, penetrate into it; and without asking whether the antiseptic hinders phagocytosis, or destroys the antitryptic power of the blood fluids, or permits or interferes with tryptic action.

This unreasoning antiseptic procedure is constantly employed. It has led to failure upon failure, and it would be a matter for wonder if it did succeed.

The *third* and last method of procedure I may call the *combined antiseptic and physiological procedure.* If we want to find a method of this sort we shall not find it by inquiring for it under this name. What we have to seek is a method which proclaims itself an antiseptic method and in this guise combats effectively, but perhaps not with full comprehension, corruptive changes in the wound.

The method of Carrel is, as I think, such a method. I would propose to show that it is a combined antiseptic and physiological method; then to survey the results obtained; and finally to consider how far the results should be credited to the antiseptic, and how far to the physiological, element in the treatment.

We have in Carrel's treatment two factors: (*a*) Dakin's antiseptic, or, as I should prefer to call it, Dakin's therapeutic agent; and (*b*) Carrel's procedure for washing and refreshing the wound surface in the intervals between the complete dressings. Now each of these factors acts not only by killing or removing microbes, but by making the conditions in the wound unfavourable for microbial growth. Let me, taking first Dakin's fluid, and then Carrel's washing procedure, try to make for you an inventory of their directly anti-bacterial, and their physiological or indirectly anti-bacterial, effects.

Dakin's Fluid.

Dakin's fluid is, as I have shown you,* a very ineffective antiseptic when it is brought into application upon microbes suspended in serum. It is also, as I have shown you, an antiseptic which has as good as no power of penetrating into albuminous fluids. It is also an extremely volatile antiseptic. When exposed in a shallow dish at blood temperature I have found it to lose four-fifths of its potency in half-an-hour, and it will, as I have already had occasion to point out, if not already quenched by contact with serum, very quickly disappear from the wound.

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* Vide supra, Experiments on Antiseptics.

Turning from the effect exerted upon microbes to the effect exerted upon the wound surface, let me recall to those of you who have seen it, that when a naked tissue surface is treated with Dakin's fluid (or for the matter of that with 5 per cent. salt solution) it is speedily converted into a bright coral-red granulating surface. That means it is converted into a defensive surface excellently well provided with new-formed blood-vessels from which active leucocytes and fully potent lymph will emerge. That is a physiological action to the good. But there are also other effects exerted. Leucocytes are affected by Dakin's fluid. Experiments show that it is destructive to phagocytosis. When we add one part of the reagent to nine of *excoagular blood* we reduce the phagocytic power of that blood by more than one-half. We abolish phagocytosis when we add one part of the reagent to four of blood. The fluid elements of the discharge also are altered in character by Dakin's fluid. Let me remind you here that we saw in our experiments on artificial sloughs that treatment with Dakin's solution hinders the digestive processes which bring about their separation. This stands in relation to the fact that the reagent exerts upon trypsin, when albumen is not there to act as a buffer, a destructive action. We have here, as you perceive, a physiological action which may quite well come into operation when a comparatively clean but tryptic wound surface is flushed. Dakin's fluid abolishes also the antitryptic power of the blood fluids. It would seem, therefore, with one hand to give protection, and with the other hand to take it away. But what really does happen is, I suppose, that trypsin and antitrypsin alike are destroyed by the flush and that afterwards in the wound a new beginning is made.

Let us follow up the train of thought here started. We may, I think, profitably ask ourselves whether if put to our election between maintaining antiseptic action continuously at the expense of physiological action, and alternating antiseptic with physiological action, we should not do well to elect for the latter policy. And we may muse whether it was not specially felicitous to have employed, as Carrel has done, an antiseptic which is very readily quenched and also very volatile, and to have applied it discontinuously. Had that antiseptic been employed by a method of continuous irrigation, phagocytosis on the face of the wound would have been excluded, and we might have had in the cavity of the wound a lymph whose antitryptic power had been destroyed.

But I have already said enough about Dakin's fluid if you have appreciated that it is a poor antiseptic; that it acts as a poison upon leucocytes and blood fluids; that its physiological action is a very complicated one; and that its beneficial effects cannot be due simply to its antiseptic action.

Carrel's Method of Irrigating the Wound.

I now come to Carrel's procedure of intercalating between the complete dressings a frequent flushing and refreshing of the wound surface and for carrying out this flushing unlaboriously. Allow me to say that we have here, I think, far the most important contribution made to surgical technique since the beginning of the war. But to that let me add, that while Carrel's procedure gives us a new and improved technique for the application of antiseptics, much more does it give us a new and improved technique for physiological treatment. In all physiological treatment the assiduous removal of corrupted and corruptible discharges is the primary desideratum.

We now return to the results of the treatment of infected wound surfaces by Carrel's method, and we may take them from Carrel's book. But it will be well, in order to keep to the kind of wound infection here under discussion, to exclude from consideration wounds complicated with fractures—for in those effective washing is difficult. And we may further, looking to the classification of wounds of soft parts in Carrel's book, exclude from consideration his class of phlegmonous and gangrenous wounds, and his class of suppurating wounds. These would correspond to wounds which have, through postponement of treatment or its interruption by transport, suffered a set-back, converting an originally light surface infection into a heavy infection with invasion of the deeper tissues. There would then fall within our purview only his class of fresh wounds of soft parts taken in hand when 5 to 24 hours old. And we learn from the data he gives with respect to these that, where there are sloughs, 15 to 20 days, and, where there are none, 5 to 12 days are required to prepare the wound for secondary suture. That gives us a measure of what can be done by what I have, I hope not without good warranty, called Carrel's "combined antiseptic and physiological treatment."

Let us consider what Carrel's results tell us. They tell us in the first place that, whatever else it is, Carrel's treatment is not in any sense a *therapia magna sterilisans*. Regarded as an antiseptic method, it is a method of "*fractional sterilisation*" requiring for the case we are considering—the simplest case of all—at the rate of 12 douches a day a series of 60 to 144 antiseptic douches. And if I am right in regarding Carrel's treatment as a combined antiseptic and physiological treatment, we have, superadded to the antiseptic, a series of 60 to 144 physiological attacks upon the microbes—each such attack starting from an atryptic condition.

The consideration of these figures leads directly to what I have to say in conclusion. While Carrel's work constitutes a very notable practical achievement, regarded as science it comes short in the respect that adequate control experiments are lacking. I do not mean that it has not been demonstrated that Carrel's treatment accomplishes what was impossible by the old system of syringing with antiseptics and leaving the wound afterwards to fill with pus. The inefficacy of that older treatment was attested by tens and hundreds of thousands of control experiments. What I mean is, that we have not in Carrel's work any control experiments with more potent and penetrating antiseptics to negative the idea that with these one could with less than 60 to 144 consecutive douches convert a light surface infection into a negligible one. And again, we have not from Carrel any control experiments with a well-thought-out physiological treatment to negative the idea that one could achieve a similar sterilisation by 60 to 144 successive physiological attacks upon the microbes, starting each time from an atryptic condition.

If we would abide in the spirit of science, every unwarranted assumption must go. We must not assume that when we have successfully combated a surface infection by a series of 60 to 144 therapeutic operations we have reached finality. And much less must we, from the fact that a treatment successfully combats surface infections, infer that it is also an effective treatment for infections which penetrate into the deeper tissues. Rather ought it to come home to us that it is impossible that there should, for quite different categories of wounds, i.e. for quite diverse conditions, be any one routine treatment.

[A. E. W.]

WEEKLY EVENING MEETING,

Friday, March 16, 1917.

THE HON. R. CLERE PARSONS, M.Inst.C.E., in the Chair.

SIR JOHN STIRLING MAXWELL, D.L.

Scientific Forestry for the United Kingdom.

My object is to convince you of two things: first, that we require more woods in the British Isles, and, secondly, that it is useless to make them except on sound scientific lines. I shall take the second proposition first. It is not the logical order, but it will be better to have the nature of the work in mind before considering whether it ought to be undertaken.

To many people the planting of woods appears so simple a thing that they wonder what part science can play in it, especially when they remember that the grandest forests in the world are the work of untutored Nature. They forget that Nature has been perfecting for ages the intricate machinery of these forests. When man dares to embark on the task of transforming bare ground into forest, he can only succeed by studying Nature's methods, and this is precisely what science does. When I speak of science, please don't conjure up visions of musty books, glass cases, dried specimens, and professors dictating mathematical formulæ. Think rather of foresters, eager in eye, brain and hand, living and working in the woods, comparing notes with one another, and valuing science precisely as they value spade and axe because it is an indispensable tool for their everyday work. Now just as forest tools have to be of good metal, so there is no room in the forest for slipshod science. Forestry, whether as a science or an art, must needs rest on the study of botany, geology, entomology and mathematics. From each of these it receives direct help. But it is more than a mere resultant of these sciences. Its peculiar study is the complex life of tree communities, its practical aim the production of the largest amount of good timber on a given area in the shortest possible time.

Before we consider examples of scientific method applied to forestry, I want you to grasp the extraordinary contrast between the position it holds in this and other civilized countries. In other countries forestry is classed with agriculture as one of the foundation industries on which the security of nations depends. It is an industry which the State has taken specially under its wing for two reasons: first, because large tracts of wood belonging to the nation or sovereign give the State a direct interest in its success; and, secondly, because it is recognized that an industry which is out of scale with the span of human life stands in peculiar need of encouragement and guidance, and really concerns the State as it never can concern any private individual. This last factor of time lies so close to the heart of the subject that we may consider it a little more closely. When some one writes a book of the psychology of different occupations, he will recognize in the forester a man who is so much outlived by the crop he cultivates that it becomes a second nature to him to think and plan outside his own lifetime—a man who can seldom hope to see, even in old age, the full result of what he begins as a boy. Sad? Yes; and foresters hate dying more than most people, but such work is inspiring. You do not pity the astronomer peering out into space or the historian diving back into time because their feet cannot follow their thoughts. You need not pity the forester because his job is bigger than himself. But certain practical difficulties attach to the cultivation of so slow a crop, and these must be faced. When seventy to one hundred years elapse between seed time and harvest, scientific cultivation is no easy matter, and early mistakes are paid for very dear. Still more severe are the financial trials. Consider the case of new ground to be planted. The man who plants may lock up his money for seventy to one hundred years, lose the rent of his land, and, in addition, have to pay rates and taxes and cost of maintenance for a property which cannot bring in a penny until such time as the sale of thinnings may help to defray these charges. The cleverest prospectus could not make this investment tempting to beings whose years are but three score and ten. Or take the case of a forest inherited as a going concern, bringing in a regular annual income as each block is cut and replanted in succession. By increasing the fellings and scamping the work of replanting, the owner can at any moment double the income for his own life at the expense of his successor. You see the point? A man may have many motives for planting or maintaining woods, but they are not the ordinary motives which, whether we like them or not, are the mainsprings of ordinary commercial effort. These considerations, together with the desire to make the most of woods under its own control, have led the State in every country, from France to Japan, to regard the care of forests as one of its primary duties.

Here the State has for several generations been innocent of such ideas. Let us see why. We have, for reasons which will appear later, no natural forests of consequence, while the forests belonging to the Crown are few and comparatively small. Yet a hundred years ago the principles of sound forestry were both understood and practised in this country, and we were little, if at all, behind our European neighbours. In the period of vigorous and enlightened rural development which marked the end of the eighteenth and beginning of the nineteenth centuries, forestry played a considerable part on most private estates, while the Government, deeply concerned in the supply of oak for naval construction, made that the main object in the management of the Crown Forests. Then, quite suddenly, British forestry fell into oblivion. With the substitution of steel for wooden ships, the Royal forests lost their importance and turned into national playgrounds. The cheap importation by steam of first-class timber from virgin forests overseas rendered the cultivation of coniferous wood unprofitable, and indeed unnecessary, except for shelter and beauty. In countries less open to the sea and dependent on wood for fuel these changes passed almost unobserved; here they conspired, with our numerous ports, ample shipping and abundance of coal, to work a complete revolution. Industries are curiously fragile things. Like virtue, they grow in use, but in repose quickly drop to pieces. The return of suitable economic conditions is not enough to revive them. It was not many years before the price of timber rose again, but in the interval the art of growing and marketing woods had been forgotten. Elsewhere the State would have seen to its revival. Here, as I have shown you, the necessary tradition was unhappily wanting.

Having seen why we are behind other countries in this industry, let us learn from a few examples how science can help the forester. You will bear in mind that abundance of straight timber is the forester's object. He obtains it by exploiting the wonderful adaptability of trees to their surroundings. An oak standing alone in the open bears little resemblance to an oak grown in a dense wood. This latter may have a clean stem 60 feet high, while in the other it would be difficult to find 10 feet of straight timber. The same thing holds true of other trees. It is the upward struggle for light of trees growing close together that produces fine timber. It is the forester's task to initiate, watch and control that struggle.

Let me take the case of a new plantation, in making which a Scots laird had the good sense to give a free hand to the experienced adviser he consulted. What did the adviser do? First, he examined an adjoining oak wood, believed to be 150 years old. He had a tree felled, counted the annual rings, and found its age to be barely 100. Oak, he said, is the tree for this exceptionally good oak soil. How was it to be planted? When a well-stocked wood is felled the soil is clear of weeds and easy to plant, being covered with the dark leaf mould, which is a precious asset of well-managed forests. But in the case I am describing a thick turf of old grass, very unfriendly to young plants, had to be reckoned with. The land was therefore ploughed and sown with oats and acorns. When the oats were cut the seedling oaks remained uninjured among the stubble. Two years later small plants of beech were sparingly added, and 800 larches to the acre, planted in rows. This plantation has never looked back. What has happened is exactly what was intended to happen. The larches have shot ahead, encouraging the upward tendency of oak and beech. Two points required attention. The larch had to be lopped back where it encroached on its neighbours, and it had to be removed altogether before it checked their growth. Part was removed two years ago, the rest this year. It was sold for pitwood, and, having at the present abnormally high prices realised £16 an acre ten years from planting, it will suffice to defray the cost of making the plantation. The oak and beech, already more vigorous than in carelessly made plantations, will henceforth grow together, with occasional thinnings, till the final felling, high forest of oak being the objective, in which a certain proportion of beech will be retained to carpet the floor with its rich fall of leaves. This example neatly illustrates one feature of forestry which is often forgotten. There is no occupation in which it is so easy to squander labour, and none in which a little labour applied at the right moment goes so far. It was not enough to make this plantation after a good design. Had either of the simple precautions I have mentioned been neglected, the result would have been a poor larch wood instead of a promising wood of oak.

We will take our next example from one of the Crown Woods. These woods, after long years of neglect, now find themselves once more under energetic and skilful management, thanks mainly to the foresight of the late Sir Stafford Howard. This photograph shows how the Crown Foresters in the Forest of Dean seized the chance of a good acorn year to restock an old wood without the expense of replanting, removing the parent trees before their shade began to injure the seedlings, and rigidly suppressing the hostile rabbit. How simple, you say. Yes, very simple, when you know how it is done: and yet, if you ask me to show you another naturally regenerated oak wood like this in Britain, I do not know where I should be able to find one. In France they may be seen wherever the oak is grown.

Even the period of neglect had good things to show now and then. Here, for instance, in the Tintern Woods, purchased by the Crown sixteen years ago, are larches planted as standards among oak coppice. This was good forestry. The forester seized an opportunity. A pure plantation of larch would not have produced stems so large and so free from the omnipresent canker. Our next example is a pure larch wood in the same forest, which has been left too long unthinned. The foresters are here studying a knotty problem. This wood, about fifty years old, is not full grown, but is worth a lot of money as it stands. It cannot be left alone. The crowns of the trees are so small that unless they are given room to expand they cannot assimilate enough food to make any substantial increase to their stems. A section of one of these trees would show annual rings diminishing in thickness since the tops became too crowded. After thinning, the annual rings of the remaining trees would quickly expand, the value of the wood increasing every year with their diameter. Now in thinning a wood like this there is grave risk from wind, and it is a nice question whether it would be wiser to fell the whole thing now and realise the present value, or thin it in the hope that the remaining trees will maintain their footing and grow into heavy timber. The decision will depend on many things besides the condition of the wood itself. A forest must give a regular yield of timber year by year, and must therefore contain woods of all ages. At Tintern woods of this age are scarce, and the Crown Foresters are anxious to preserve this one if they can. They are leaving nothing to chance. You see them in this photograph examining a sample block, every tree in which is numbered, entered in a book and measured at regular intervals. They are trying various methods of thinning in various plots, and they know to a square foot what increment each is producing. Twenty years hence there will be some noble stems here provided the wind spares them. You will hear people say that we are more subject to wind than our Continental neighbours; I do not think this is true. The Continental foresters take care to guard against the dangerous winds, and all their plantings and fellings are designed with this object. When these plans are disturbed the wind makes havoc. In the Eberswald, near Munich, a few years ago great stretches of the forest were destroyed by the nun moth, and had to be felled out of their order. The wind found entrance, and 2000 acres were laid flat in the gales of the succeeding winter.

I pass now for a concluding example to quite another type of work—the planting of waste land—and ask you to accompany me to a loch in Inverness-shire 1270 feet above sea-level, and close to the watershed of Scotland on its western side. Here, in plantations begun twenty years ago, we may learn something about the limits to which afforestation can be carried. In order to secure the forest area required with the least disturbance to other interests, it will, as a rule, be necessary to devote whole subjects to afforestation, that is to say, whole sheep farms, deer forests, or whatever the subject may be. No one proposes to plant arable land, but any such land attached to the forest subjects will be required to provide holdings for the forest workers. Even if the most suitable subjects are chosen, the whole area will seldom be of equal value, and it is important that the high and poor ground should not be wasted if, by taking pains, it can be made to grow good timber. The ground in this case was poor glacial drift covered with peat, in which were imbedded the roots of vanished forests of Scots pine. The only surviving trees were birch, mountain ash, black alder and bird cherry, with a few willows, all sadly distorted by snow. The new plantations were made with good advice, or rather, with advice which would have been good elsewhere. Here it was not good, because it had not correctly gauged the problem to be solved. No one doubted that the Scots pine was the best tree to plant. Accordingly it formed the matrix of the plantations, larch being introduced where the ground was suitable, with common spruce in the wetter places, and here and there groups of various American and Japanese conifers. The result was a conspicuous failure. Then the owner began to make discoveries. He heard of a plan devised by the Belgian foresters for planting the high moors near Spa. He went to Belgium to see what they were doing. The first thing he learnt was that the Belgian foresters had discarded the Scots pine as hopeless for this sort of ground. They showed him a plantation of this species forty years old and less than 20 feet high, the failure of which had long deterred the Belgian Government from making any more plantations in that region. Then they showed him a flourishing spruce wood which fringed the same moor. Can the soil, they asked, be good on one side of that straight fence and bad on the other? Is it not clear that the spruce, if only you can get it established, will grow on this moor though the Scots pine will not? Now in the Inverness-shire plantation the spruce had been reckoned a failure even among the failures. True, after ten years of obstinate sickness, its complexion had slowly changed from yellow to green, and it was beginning here and there to add a few cautious inches to its stature, but at that rate forestry brings neither pleasure nor profit, and the owner and his forester had given up the spruce as a bad job. Not so the Belgian foresters. Once assured that the spruce would grow, they set about devising means to establish it quickly, and this is the system at which, after many experiments, they arrived. The ground had to be drained. A large turf taken from the drain was laid on its back in the place where each plant was to go. When the time came to plant, a hole about the size of a No. 4 flower-pot was cut in each turf with a trowel-shaped spade. In this the plant was placed with two handfuls—small handfuls, for the planters were women—of sand or gravel mixed with basic slag in the proportion of 7 of sand to 1 of slag. I cannot tell you why, but the plant derives no benefit from the slag unless it is mixed with mineral soil. The quantity of manure is very small. It tides the tree over its initial difficulties, so that it makes and ripens a good growth the first year, while the turf keeps it clear of weeds. Possibly it assists the development of friendly bacteria in the soil. The decaying turf and herbage beneath avail to support the young plant over the second year. The third, it begins to take hold of the ground, sweetened by this time by the action of the initial draining. I cannot show you photographs of this work in Belgium, but I can show you the faithful imitation in Scotland. Sitka spruce, a tree from the western coast of North America, was the species employed in this case. The trees were planted as two-year seedlings about 3 inches high. Two years later they showed some progress. In five years they began to get into their stride. Now, in the eighth year from planting, many of them are 10 feet high and beginning to make rapid growth—a fair result on poor soil and 1300 feet above the sea.

The common spruce, which alone is used in Belgium, responds well to the same treatment, though it does not grow quite so rapidly.

The use of manures in forestry may serve as a sample of questions which await investigation. The views of British foresters on the subject are vague and inconsistent. Nearly all use manures in one form or another in their nurseries. Nearly all are opposed to their use in planting. Some will tell you that they have no effect, and it is, indeed, very easy to apply them in such a way that they can have no effect. Others will tell you that their use is radically unsound, because they are soon exhausted. Does it matter how soon they are exhausted if they have done their work? Who grudges the motor a turn of the handle at starting? I have in my mind an experiment in which a few handfuls of sand mixed with basic slag were applied to certain rows in a plantation, once more of Sitka spruce. The manured trees are now 10 feet high, while their neighbours, left for comparison without manure, are less than 2 feet. The recovery of these last, I believe, to be only a matter of time, but they will always be five to ten years behind the others. In forestry time is money.

People imagine, very naturally, that the native trees must be better suited to our soil and climate than those imported from other countries. That view is held on the Continent of Europe with some reason. As regards this country it is quite mistaken, and is especially untrue of the conifers, which concern us most, since nine-tenths of the timber we consume is coniferous. In the glacial period the ice banished all vegetation from this part of Europe. When plant life returned after the retreat of the ice, only three conifers came back to what are now the British Isles—the Scots pine, the yew and the juniper, of which but one, the Scots pine, has any forest value. Probably our climate was then drier and warmer, and therefore better suited for the Scots pine, and less suited for other conifers than it is now. We all love the native pine for its beauty. It still grows well in the eastern counties from Norfolk right up to Cromarty Firth, though not so well as in the drier climate of Saxony. Here and there fragments of the old forests still fledge the Scots hills. But on our western watershed the tree is undoubtedly dying out. In large areas where it was once common it has completely disappeared, no trace remaining except in place names and roots embedded in the moss. Yet in the unobservant forestry of the nineteenth century, when high or bare ground had to be afforested, this tree was persistently planted simply because it was indigenous. Its failure has given such enterprises a bad name. The larch, spruce and silver fir, exotics from the mountains of Central Europe, are all better for such work. These trees, and still more the conifers from the western coast of North America, grow good timber at altitudes which are quite beyond the reach of our native pine, and it is possible that others from Japan and Western China will succeed as well.

In the plantations of which I have been speaking, the Scots pine is broken by snow and browned by spring winds which deprives its foliage of moisture, which the roots cannot replace while the ground is frozen. After twenty years' growth it is not more than 20 feet high, and often less. The height growth of Sitka spruce and Nobilis fir is nearly double. Their branches bend to the snow and decline to yield their moisture to the east wind. They stand uninjured among the debris of the pine. The Nobilis fir is a tree specially adapted to moorland planting, because it hates lime and thrives on peat. In any scheme of afforestation deserving to be called scientific, one of the first steps will be thoroughly to test the value of the newer exotics as agents capable of extending plantations to soils and elevations which it would be useless to plant with our native trees.

In nothing has the unscientific character of recent forestry in this country been so conspicuous as in its relation to exotic trees. The spruce and larch have been with us since the 17th Century, but even now their habits are still very far from being generally understood. The larch is essentially a mountain tree, only growing vigorously where the soil is stony, the drainage quick, and moisture, whether from rain or melting snow, abundant in spring. It has been planted in this country by the million in situations as unfriendly to the tree as they are favourable to its enemy, the canker fungus. What disappointment would have been saved if we had taken a hint from the French, who never employ it except on the stony slopes of their mountains.

The spruce, as we usually see it in this country, is a tree of no value, because the wood is full of knots. Properly grown in close plantations it produces a very large volume of light, easily worked timber, which is in constant demand and which, creosoted, makes good railway sleepers. It can be grown to perfection in England, Scotland and Ireland, as may be seen in the photographs which adorn the noble volume of Mr. Elwes and Dr. Henry. Yet if you search Britain from Land's End to John o' Groats I doubt whether you will find more good spruce woods than you can count on your fingers.

The Douglas fir has been cultivated here since 1827, but well grown woods of it are still scarce. One of the oldest and best known, at Taymount in Perthshire, is only fifty-seven years old, and grows on soil far from ideal. At Benmore in Argyllshire there is a much larger wood, planted thirty-seven years ago on ground very steep and rocky but evidently congenial. These suffice to prove that the species can on suitable sites yield heavy crops of good timber in a shorter time than any other tree.

The Sitka spruce, introduced in 1831, has not yet been tried on so large a scale, but promises results equally remarkable. At Craigo in Forfarshire there is a group sixty-six years old and well over

100 feet high, with tall cylindrical boles 10 feet in circumference. A tree of the same species at Murthly in Perthshire was measured independently in the autumn of 1916 by Dr. Henry of Dublin and Mr. Jackson of Kew, and proved to be 126 feet high and 13½ feet in girth. Another at Stanage Park in Wales, growing 800 feet above the sea, is reported by Mr. Rodgers to be now 124 feet high and 12 feet in girth. Yet this is a tree which many foresters still hesitate to plant because frost often checks it in youth. Its power of recovery from such attacks should reassure the most timid. If the leading shoot is destroyed it promptly turns up a side branch to form a new leader.

These excursions in the woods have left us little time in which to discuss the reasons why a policy of afforestation has become a national necessity. Argument is almost superfluous after the Prime Minister's statement in the House on February 23, 1917. He told us plainly that timber absorbs more shipping than any other import; that the situation is in consequence very grave; that in the effort to reduce imports the problem of timber must be attacked before any other. The fact is that dependence on imported timber has in this war been like a millstone round our necks. In some cases, as the late Prime Minister pointed out in a speech to the miners, we have only been able to obtain it from neutral countries on condition of sending coal in exchange. Beggars cannot be choosers, or such a condition in time of war would never have been tolerated. You will notice how the German Chancellor in booming the submarine blockade invariably couples timber with foodstuffs. He has every reason to do so. In increased prices, freights and insurance, and cargoes sunk, we have already wasted in the last three years between thirty and forty millions of money on imported timber. The timely expenditure of a fourth of that sum, beginning seventy years ago, would have established in this country all the woods necessary for our security. The French forests are suffering, and must suffer, because we have failed to take the precautions long ago taken by every other country. Only thus can the wants of our Army be supplied. Again, all through the war the Welsh coal-mines have been indebted for their pitwood to the foresight of our Allies in afforesting the waste lands of the Landes near Bordeaux.

Have we, you will ask, made full use of the timber growing in this country? That question cannot be answered till the end of the war. So little was it considered at the beginning that we had been at war more than a year before steps were taken by the Government to encourage the use of home-grown timber. By that time half the skilled woodmen had gone to the Army. Their places have to some extent been taken by regiments of Canadian lumbermen, which are working in this country for the Government. We have much to learn from them, especially in the matter of transport. With more labour the output from the home woods might have been much larger. It is only fair to say that no one foresaw what enormous quantities of timber would be consumed by modern military operations, and that there existed no reliable survey of the timber available. No survey has yet been made, nor has any record been kept of the amount felled. Under these conditions it is idle to speculate how much remains. We can see that the quantities of standing timber, especially of pitwood, are happily still considerable. Perhaps it is lucky that they were not tapped earlier, since all that remains may be wanted.

One is driven to the conclusion that the least we can do in the interests of national safety is to increase our area of wood sufficiently to make the country independent of imports for at least three years in an emergency. These islands, in proportion to their size, have less than one-fourth of the woodland possessed by France and Belgium, less than one-sixth of that possessed by Germany. In making ourselves independent of imports for a short period we are not confined by the rule which limits the annual fellings in the forest to a volume equal to the annual growth. We can, in an emergency, at the expense of the future, but without devastating our woods, make in three years the fellings which in normal times would be spread over fifteen. On this basis, the addition of a million and a half acres to our existing woodlands would be sufficient for safety, assuming that the areas felled during the war were replanted and kept properly stocked. Even so, we should still be poorer in timber than any other European country, except Portugal. A danger period is inevitable, when we shall be very vulnerable by any power which can cut off overseas' timber. Every year we delay making provision is a year added to that period.

Though the experience of the war will no doubt supply the principal motive for adopting a policy of afforestation, there are two other considerations, either of which might well be conclusive. The first is the precarious nature of the world's supplies of coniferous timber even in time of peace. The consumption of this class of timber is steadily increasing all over the world. For nine-tenths of our supply we depend on the virgin forests of foreign countries, which are rapidly being depleted. Unless a radical change comes over the management of these forests, and especially unless the ravages of fire are checked, timber will be very difficult to procure in sufficient quantities seventy years hence. Steps to meet that emergency must be taken at once. Unless the forests of Canada can be safeguarded and made available, provision will have to be made in the United Kingdom on a much larger scale than that indicated above.

The other consideration is perhaps more in harmony with the aspirations of the moment. These islands contain vast areas of rough pasture little better than waste and almost uninhabited. Can no better use be made of them? Every European country has been faced by this question, and every country but this has long ago found the answer in afforestation. A scheme such as I have indicated would not absorb by any means the whole of the rough pasture, and probably not even more than a quarter of what is plantable, but the method of carrying it out would require most careful thought. The knowledge of forestry in this country resides partly in experts employed by the Government and teaching centres and in the officers attached to the Crown Woods; but to a large extent, and perhaps in its most practical form, it resides in the owners of private estates and their foresters, who are responsible for 97 per cent. of the woods in this country. We have seen that the great risk attending privately owned forests is the temptation to sacrifice the future to the present, to secure an immediate gain at the expense of great eventual loss. The State does not usually excel in economy, but the experience of other countries shows that it can, on the whole, conduct forests better than private individuals. Thus in France and Germany the production is found to be greater in the State forests than in private woods, while the communal woods, managed by the State for the villagers with many concessions to their wishes, stand halfway between. The situation seems to point to a double advance on parallel lines—first, the acquisition and planting of land by the State, and, second, the encouragement of planting by private individuals, in return for some guarantee that their woods will be properly maintained. What does encouragement mean? It means skilled advice in the making of working plans and schools where working foresters can be trained. It also means that in some form or other the State will have, for some years at least, to make up the difference between loss and reasonable profit on private plantations. It is impossible now to calculate how far plantations made after the war will pay. The conditions do not appear favourable. Prices, bank rate and wages are the chief factors, and all are uncertain. One thing only is certain—that the nation must have the woods necessary for national safety even if it has to pay for them.

Quite apart from the direct profit or loss, the indirect gain to the nation by afforestation will beyond doubt be very great. It is inevitable that private individuals should mainly be guided by the question of profit or loss, but this is the only country in which the State has regarded afforestation wholly or even mainly from this point of view. In other countries, not because they are less businesslike, but because they are more businesslike, the motive for afforestation has been found, not in the hope of direct profit, but in the increased productiveness of the land under forest and the increased population it is able to support. Some people find it difficult to keep these questions distinct, but they really are so. Whether a wood is profitable or the reverse to a planter there is no shadow of doubt that it enriches the nation. Provided it grows reasonably well, each acre will produce annually a ton of timber; every 100 acres will support a family; for every £100 of timber sold standing an equal sum will be paid in wages for its felling and conversion. To ascertain the value to the State of the plantation, this state of things must be compared with the rough grazing where a single shepherd is employed to 500 or 1000 acres, and the annual production of each acre seldom exceeds 5 lb. of mutton and ½ lb. of wool, or with the deer forest which produces about 1 lb. of venison per acre.

There are three matters which must be taken up in advance of afforestation, and I trust public opinion will support the Government in getting to work on them at once, without waiting for the end of the war. The first is a survey, to ascertain the best forest sites; the second, the provision of plants in large quantities; the third, the training of forest officers. The last is the most important of all, since scientific method is the first condition of success.

[J. S. M.]

WEEKLY EVENING MEETING,

Friday, March 23, 1917.

Sir William Phipson Beale, Bart., K.C. M.P. F.C.S.,
Vice-President, in the Chair.

Edward Clodd, J.P.

Magic in Names.

Among all lower races, and among the superstitious in higher races, there is found belief in a vague, impersonal power which acts through both the living and the non-living. It is the stuff through which the sorcerer, whether deceiving or self-deceived, exercises control over persons and their belongings to their help or harm, and also control over invisible beings and occult powers. As black magic, it works maleficently; as white magic, beneficially; in each case through both the tangible and the intangible. The savage believes that the sorcerer can cast a spell upon him through his nail and hair cuttings, his saliva, his blood, portrait, etc., and through the intangible, as his shadow, reflection, and, notablest of all, his name, which to him is an integral part of himself. Hence the world-wide custom of name-avoidance; the precautions to conceal the name and, *vice versa*, the dodges to discover it; hence, also, the taboo on the names of relatives, of the dead, of sacred persons and of spirits, through ascending stages, to the high gods.

From birth to death the name-taboo works ceaselessly. Sanctity or secrecy attends the ceremony of name-giving. Borneans, Lapps change, and, in ancient times, Jews changed, the names of the sick to deceive demons or to elude death; but it is in savage initiation rites, which have been called "the lineal ancestors of confirmation," that the giving of a new name is found to be common.

On arrival at puberty the youth is sent into retreat and is re-named; as with the Roman Catholic and Buddhist monks, and with the nun who takes the black veil, the old name is effaced and a new name is taken. The name as a part of the person has amusing example among the Kwakintl Indians of British Columbia. A man in debt can put his name in pawn, remaining anonymous till he pays up. Believing that luck or ill-luck attaches to certain names, Scotch fisher-folk use all sorts of roundabout names for their catches, corresponding to which are the euphemisms which barbaric hunters apply to their quarry, and Malayan miners to metals in the belief, e.g., that tin is alive and moves about, and must be called by another name to obtain it. A like belief in their personality explains the propitiating and flattering names applied to diseases, as when the Borneans call the smallpox "jungle leaves," and when the Slavs call the fever demon "godmother."

In family life the name-taboo is active; people related by blood or marriage, notably in the case of mothers-in-law, avoiding mention of one another's names. But it is with the ascending rank of persons that the taboo gathers force. From the Far East to Dahomey there prevails the custom of speaking of emperor, king or chief by some other name than his rightful one; and the like applies to priests.

World-wide is the prohibition or the reluctance to name the dead, lest thereby their ghosts should appear, the bereaved sometimes changing their own names so as to baffle the ghost should he return. A group of folk tales, exampled by the German "Rumpelstilzchen" and the Suffolk "Tom-Tit-Tot," have at their core the barbaric belief that if the name of the demon can be found out he becomes powerless, and the like applies to the gods. Râ, the Egyptian sun-god, lost his power when Isis beguiled him into telling him his secret name. The name of Rome's tutelar deity was kept secret as her safeguard, and to this day it remains undiscovered. Chaldean and Jewish books of magic teem with formulæ concerning the mystic names of the god, and only four years ago a number of monks were ejected from Mount Athos for holding the heresy that God's name is a part of Himself.

Among the ancient Jews severe penalties were attached to the utterance of the name Yahwe, or Jehovah, and in Moslem belief Allah is only an epithet for the god's name. On the other hand, magic power is wrought through the invocation of the sacred name. To this the Gospels bear witness in their reports of the expulsion of demons and the healing of the sick in the name of Jesus, and the Early Fathers of the Church record what wonders were performed in the name of the Trinity. To the survival of this belief, cure-charms, inscribed amulets and the like supply proof.

What magic power is believed to inhere in words themselves detached from persons has example in the creative formula, notably in the Egyptian, whereby the god comes into being by uttering his own name, and in the Hindu, the power of the "mantram" being such that it can enchain the gods or make them tremble. Only by mystic passwords can the soul, according to ancient Egyptian belief, secure admission to dwell with Osiris triumphant, and if by mishap the dead man's name be lost, he becomes extinct.

The survey of a subject which can draw examples from every grade of culture brings home the fact of the persistence of primitive ideas, and adds its crowd of witnesses to that continuity of the spiritual which has its correlate in the physical.

[E. C.]

WEEKLY EVENING MEETING,

Friday, March 30, 1917.

COLONEL EDMOND H. HILLS, C.M.G. R.E. D.Sc. F.R.S.,
Secretary and Vice-President, in the Chair.

J. H. JEANS, M.A. F.R.S.

Recent Developments of Molecular Physics.

As the subject of my discourse this evening I propose to take the surprising developments in the fundamental conceptions of physics which have been forced upon us in the last few years. In the closing year of last century, Lord Kelvin delivered a Friday evening discourse, taking as his title "Nineteenth Century Clouds over the Dynamical Theory of Light and Heat." He said: "The beauty and clearness of the dynamical theory which asserts light and heat to be two modes of motion is at present obscured by two clouds." The cloud over the dynamical theory of light centred round the question of the motion of the earth through the ether; that over the dynamical theory of heat was concentrated about the famous theorem of Equipartition of Energy. In the seventeen years which have elapsed since then, the attempt to remove these clouds has led to the introduction of two new principles—or perhaps it is better to speak of them as theories, since they are both still in a tentative stage. The Theory of Relativity has been introduced to remove the first cloud, and the Theory of Quanta to remove the second.

These are, perhaps, the two most revolutionary theories that have been seriously considered by science for some centuries. They are revolutionary especially because they involve a new philosophical and metaphysical outlook which implies a complete break from that formerly held. It need hardly be said that the new theories are still very far from having achieved universal acceptance, but whether they ultimately prove to be true or not, the mere consideration of them has beyond dispute done a great deal towards suggesting a way out of the *impasse* in which molecular physics found itself at the end of last century. The two clouds of which Lord Kelvin spoke are not yet completely dissolved, but rays of light are appearing here and there, and the vista beyond, interpret it in any way we will, is certainly one of the most interesting and fascinating that has ever spread itself before the physicist.

Let us begin with a very brief consideration of the first cloud. The astronomical phenomenon of the aberration of light is known to all. To catch the light from a given star, the astronomer must not point his telescope towards the position of the star, but in a direction obtained by compounding the velocity of light with the velocity of the earth in space. The principle is substantially the same as that on which, when rowing through a strong current, the nose of the boat must be pointed up-stream above the spot it is desired to reach. If light is brought to us in the form of ether waves, the phenomenon of aberration shows that the earth must be moving through a stagnant ether, so that to us on the earth there must appear to be a current of ether streaming past the earth.

It is natural to attempt to measure the velocity of this stream, and so determine the earth's absolute velocity in space. In the famous Michelson-Morley experiment, which was designed to this end, a beam of light was split into two parts. One part is sent up the ether stream and comes down again to the starting point after reflection by a mirror, while the other half is sent an equal distance across stream, and again comes back after reflection. Thus the total length of path is the same in each case, and if the earth were at rest in the ether, the time occupied in going and returning would be the same for each ray. But if there is a stream of ether moving past the earth, it is readily seen that the second half of the beam must gain in time on the first, for the time lost in moving against stream by the first ray is not fully compensated by the gain in time when moving with the stream. The experiment was arranged so that any difference in the time of the two rays would show itself in the formation of interference fringes, and this difference would give a measure of the earth's velocity through the ether. But the experiment, repeated with all possible checks and refinements, refused to disclose any motion of the earth through the ether at all, and other quite different experiments designed to the same end gave one and all precisely the same reply.

Thus, assuming that light consisted of waves in the ether, experiment and observation led to the conclusion that the ether must be at rest, and the earth at rest in the ether, so that the evidence seemed, if strictly interpreted, to lead back to the geocentric universe of pre-Copernican days. This was the cloud over the dynamical theory of light.

From the attempts of Lorentz and Einstein to unravel this contradiction, the theory of relativity has arisen. It says in effect: "The existence of an ether is conjectural, while the results of the Michelson-Morley and other experiments are certain. Let us abandon the dynamical interpretation of light, which is based on the conjectural existence of an ether, and examine what laws are obtained by starting from the hypothesis that all attempts to measure the absolute velocity of the earth, or of any other mass, must necessarily lead to a *nul* result." On this basis, supplemented by certain extensions and generalisations, it is found possible to construct a definite and consistent system of laws. They of course differ from the dynamical laws based upon the supposed existence of an ether, but they differ only where relative motion is involved, because the relativity theory makes its laws agree with those of ordinary dynamics when there is no relative motion. Wherever the old and the new theories differ, an appeal to experiment has so far invariably decided in favour of the new theory of relativity. The most recent triumph of the new theory is of such great interest that it may perhaps be mentioned in some detail.

Gravitation has always stood aloof from other physical phenomena. Since Newton formulated the law of the inverse square of the distance, nothing has been added to the law and nothing taken away. Except that it represents a natural weakening of gravitational effect by spreading out in space, no explanation of the law has ever been given, nor even a plausible conjecture as to the relation between gravitation and other physical agencies. Recently Einstein has found that the Newtonian law is inconsistent with the postulates of his general relativity theory. On amending the law so as to conform to these postulates, it appears that the orbit of a planet about the sun ought no longer to be a simple ellipse, as it was under the Newtonian law, but rather an ellipse slowly rotating in its own plane. For instance, the orbit of Mercury ought to revolve at a rate of about 42·9″ per century. Now, for some time one of the outstanding problems of astronomy has been the explanation of the irregularities in the orbit of Mercury. After allowing for all known causes of irregularity, there was found to be outstanding a secular advance of the perihelion, or more simply a slow rotation of the orbit, of amount almost exactly equal to the 42·9″ per century predicted by the relativity theory of Einstein.

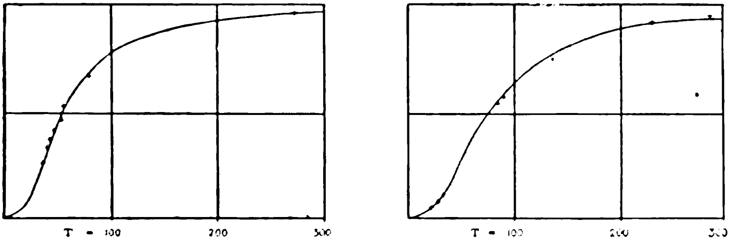
To sum up, then, it is clear that the first of our two clouds has been dissipated by the theory of relativity. This is not surprising, for the theory was in effect designed for this special purpose. But it is important that the cloud has been removed without another one appearing to replace it. Indeed, other clouds have also been removed in the process, such as that surrounding the orbital motion of Mercury. The relativity theory has succeeded, as we have seen, by relegating the ether to a position of absolute unimportance. Whether the ether exists or not we do not and cannot know, but the relativity theory indicates that everthing happens exactly as if the ether did not exist. The new theory is concerned with the discovery and formulation of laws rather than with their causes, and so makes no claim to pronounce on the existence of an ether; but it is clear from the experimental evidence of the Michelson-Morley experiment, that if it is finally necessary to call upon an ether to interpret phenomena, this ether will be something quite different from what we imagined it in the past. In the meantime, the attitude of the relativity theory to the ether is that it has no need for that hypothesis.

The second cloud, over the dynamical theory of heat, is in effect the theorem of Equipartition of Energy, discovered as a mathematical theorem by Maxwell in 1857. The dynamical theory of heat asserts that heat is a mode of motion. Every body has a certain number of capacities of internal motion, or to use the technical term, degrees of freedom, the energy of motion of these degrees of freedom forming what we call the heat of the body. The theorem in question begins by assuming that the motion is determined by laws of the type of Newton's laws of motion, and on this hypothesis it shows that, when any temporary disturbances have passed away, the energy is, so to speak, fairly rationed out amongst the different degrees of freedom. It is not proved that one degree of freedom will have just one ration, but if we take any large group, no matter how selected, their average amount of energy will always be exactly one ration. We can state the matter in a different way by saying that the energy is distributed at random amongst the different degrees of freedom: none of them gets any preferential treatment. The simplest instance of the truth of the theorem is, perhaps, found in the law that the atomic heats of all elements is the same. On the average an atom of silver has just the same energy as an atom of aluminium at the same temperature. The atom of silver is four times as massive as the atom of aluminium, but the energies are made the same by the velocities in silver being just half of those in aluminium.

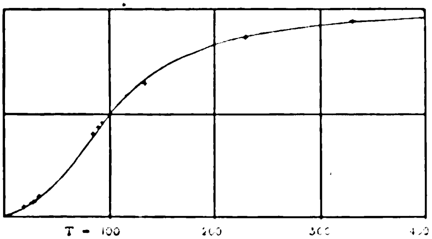
Another aspect of the meaning of the theorem of equipartition deserves attention. A column of air, say an open organ-pipe of 16 ft. length, can sound not only its own note, but a number of harmonics as well—one in the first octave above the fundamental note, two in the octave above, four, eight, sixteen, and so on, in the succeeding octaves. The degrees of freedom of the air inside the pipe may be thought of as arising from the possibility of motion in space of the molecules of the gas, but they may alternatively be thought of as the possibility of the pipe sounding its fundamental note and all the harmonics, down to those of the very shortest wave-length, comparable with the distances apart of the molecules in the gas. The principle of equipartition now requires that when any exciting agency has died away, and only pure heat-motion remains, the energy shall be distributed equally among all its notes. It can in point of fact be shown by mathematical demonstration that the random heat-motion of the molecules inside the pipe can be resolved into a system of wave-motions such that the fundamental note and all the harmonics have, on the average, exactly the same amount of energy. Thus, so long as the theorem of equipartition remains true, we may regard heat-motion as a musical effect, produced by sounding all the notes which the system is capable of sounding, with equal energy.

A number of instances, such as that of the atomic heats of metals already referred to, prove that the theorem is true in nature, at least within certain limits. But the limits are easily discovered, and it is readily seen that the theorem is not of universal applicability. For example, in the instance just taken, the atom of silver has four times as many electrons in its structure as the atom of aluminium, so that its internal structure has four times as many degrees of freedom. Why, then, does it not get four times as much energy? This and similar cases of failure of the theorem formed the cloud over the dynamical theory of heat.

A good deal of information can be obtained by investigating in what ways the energy is distributed in the cases in which the theorem of equipartition is found to fail. In the case of solid elements such as silver and aluminium, it is found that all the heat-energy resides in the motions of the atoms as a whole; the internal motions of the electrons get none at all. The same is true in a monatomic gas, such as helium or mercury vapour, as is shown by the fact that the ratio of specific heats is $1\frac{2}{3}$. In a diatomic gas at ordinary temperatures, three-fifths of the total energy resides in the motion of translation of the molecules, while the whole of the remaining two-fifths resides either in the motion of rotation of the molecules or in



Silver Copper



Aluminium

Fig. 1.

the to-and-fro motion of the two atoms of which the molecule is formed, probably the latter.

The instances so far mentioned are sharply divided into cases of complete success of the theorem and case of complete failure. But every scientific investigator will recognise that our chances of unravelling the causes of failure will be enormously improved if we can find a case of gradual transition from truth to failure.

An interesting case of failure of exactly this kind has been disclosed by recent investigations on specific heats at low temperature. If the principle of equipartition held, even as regards the atomic motions in a solid, the atomic heats ought to be the same for all elements. They are so at high temperatures, but there is a steady falling off as the temperature decreases. The specific heats of silver, copper, and aluminium at low temperatures, as measured by Nernst and Lindemann, are shown in Fig. 1.

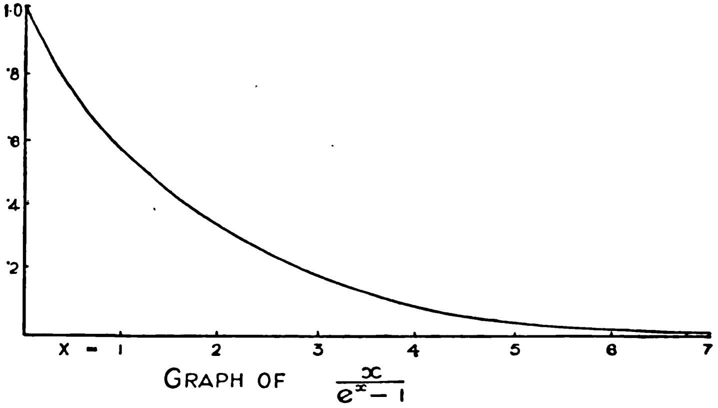


FIG. 2.

Debye, attacking the question mathematically, has shown that the observed values of the specific heats, both for these and other substances, are exactly what they would be if the energy were not rationed out equally amongst the different waves, as demanded by the principle of equipartition, but according to the law

$$\frac{x}{e^x - 1}$$

where x stands for $h\nu/RT$, ν being the frequency of the vibration, T the absolute temperature, and R and h being constants. Fig. 3 shows the theoretical curve deduced by Debye from this law, together with the observed values of Nernst and Lindemann for three elements. We now see that in heat motion at low temperatures the "sound" is one in which all harmonics do not sound with equal energy; the higher harmonics get nothing like the share of energy allotted to them by the theorem of equipartition, and the energy tends to concentrate in the vibrations of lowest frequency.

The formula just given has the very special significance that it also expresses the partition of energy amongst the different vibrations in the spectrum of a normal black body, as given by the well-known and now generally-accepted formula of Planck. Indeed, it is probable that for dynamical reasons the partition of energy in the black body spectrum must form an indicator of the partition of energy in the black body itself, out of which the spectrum originates. Planck has proved that this is necessarily the case; but his argument is weakened, and to some extent invalidated, by the circumstance that the proof is based on the Newtonian laws of motion which we are now discarding.

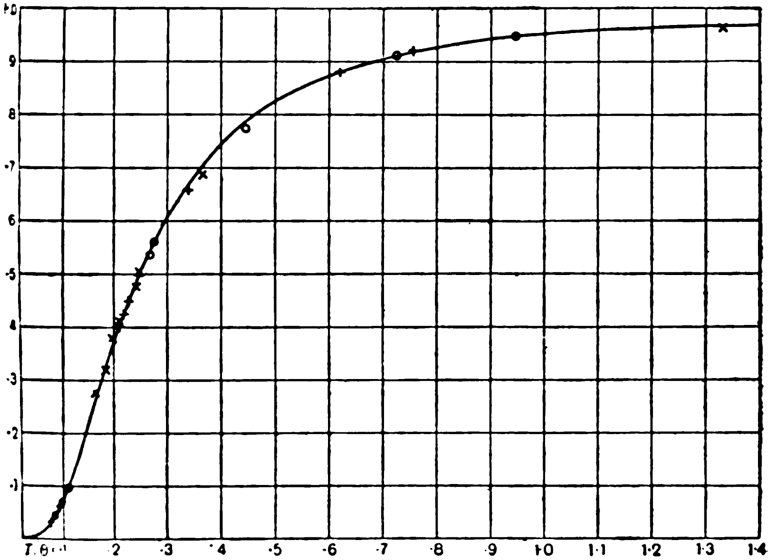


Fig. 3.

Atomic Heats at Low Temperatures (+ Aluminium, o Copper, × Silver).

In general, however, we may say that in every case examined the partition of energy in vibratory motions of all kinds must be supposed to be that given by the above formula. When x is small, covering the case of slow vibrations, the formula is of unit value, and the partition is one of equality; on the other hand, the formula shows that vibrations of high frequency get no energy at all, so that here we get the extreme case of failure of the law of equipartition. It is extremely important for us that the theorem does fail in this way, because if all the heat energy in the world were to distribute itself equally over all the degrees of freedom in the world, as required by the theorem of equipartition of energy, everything would become frozen and dead within a small fraction of a second.

Although the view has been opposed in the past, there is now, I think, no room for doubt that the failure of the theorem of equipartition of energy must be interpreted in the most obvious and direct way. The theorem is true subject only to the assumption that the motion is governed by Newton's laws; as a matter of experiment the theorem is found not to be true of high-frequency vibrations; therefore the motion of high-frequency vibrations is not governed by Newton's laws. We must search for a new system of dynamical laws which shall give the observed partition of energy.

The formula $x/(e^x-1)$, which is believed to give the true partition of energy, was originally deduced by Planck from theoretical considerations; but the underlying conceptions were of such a strange and novel nature that at first they gained but little credence. But when the law of partition is known, it becomes merely a mathematical problem to discover what laws of motion will result in this law of partition. In 1911 I showed that this law of partition could only result from laws substantially identical with those already assumed by Planck. Shortly after, Poincaré announced the same result, with the important addition that the main nature of the laws would not be altered by a slight variation in the observed law of partition. Planck had assumed that energy was transmitted not by continuous processes but by a system of jumps and jerks. It appears that the mere fact that the energy is not divided equally among the different vibrations is sufficient to show that there must be discontinuities of some kind in the fundamental laws of motion. The particular type of discontinuity which is found necessary to lead to the observed formula $x/(e^x-1)$ is expressed by Planck's equation

$$\epsilon = h\nu.$$

Here h is the same constant as occurs in the value of x, ν is the frequency of the vibration in question, and ϵ, equal to $h\nu$, is found to be of the same physical dimensions as energy, and may be spoken of as the "quantum" of energy associated with a vibration of frequency ν. The law of discontinuity is such that the vibration can only gain or lose energy by whole quanta. Thus a vibration of frequency ν may have no energy at all, or one quantum, or two quanta, but cannot for instance have half a quantum or $1\frac{1}{2}$ quanta—the energy changes by discontinuous jumps.

It appears, then, that we are brought to the contemplation of a universe in which the ultimate motion is of a discontinuous nature. The supposed continuity of nature must be only an illusion ; motion when seen on a large scale is continuous, but is resolved into discontinuity when we imagine it viewed under a sufficiently high-powered microscope. Such revolutionary conceptions will be made mentally more palatable to us if we can find direct evidence of the "jumps" in question, and a good deal of such evidence exists.

Perhaps the most striking, although not the most direct, evidence is provided by Bohr's theory of line-spectra. We know from the researches of Rutherford that the hydrogen atom consists of two constituents—a positive electron and a negative electron—circling round one another. Bohr assumes that only a limited number of orbits are possible ; the two electrons may circle at distances such that the motion possesses one, two, or any integral number of the quanta of energy corresponding to the frequency of rotation, but at no other distances. Sudden drops from one of these distances to another can take place, and when this happens the energy set free leaves the atom in the form of one quantum of monochromatic light, the frequencies of these bundles of light giving the line-spectrum. The line-spectrum has always defied interpretation in terms of the old mechanics ; indeed, the old mechanics made it impossible that a line-spectrum could occur at all. Bohr's theory has given a brilliant explanation ; his theory predicted the position of the lines exactly, and further predicted the existence of other lines in the infra-red which were not known to him when he published his theory, but were subsequently discovered by Lyman. The theory also predicted a large part of the helium spectrum, and a comparison of this spectrum with that of hydrogen enabled Fowler to determine the mass of the electron to an accuracy at least equal to that of the best of previous determinations.

Einstein has supposed that when the quantum of energy is set free in the form of radiation from radiating matter it does not spread out in space, but remains as a compact bundle of energy. We must, on this view, think of radiation not as waves spreading out in a sea of ether, but perhaps rather as fishes swimming out into a sea of— we do not know what. If this is so, we might be able to obtain very direct evidence of the existence of these fishes by spreading nets to catch them. Suppose we spread nets of varying meshes, say two, four, six, eight inches, and set free what the quantum theory tells us ought to be five-inch fishes, and what the old mechanics tells us ought to be waves in our sea. Suppose we find that the six-inch and eight-inch nets are unaffected, while the two and four-inch nets show holes in each case of five inches. Suppose further that millions of what the quantum theory pronounces to be one-inch fishes have no effect, while even the smallest number of what the quantum theory calls three and five-inch fishes are found to make holes of the corresponding size—shall we not be justified in supposing that we are not altogether on the wrong track in believing that the fishes really exist?

Allowing for the necessary imperfections of an analogy, evidence of the kind just described is provided by the photo-electric effect. High-frequency light falling on a clean metallic surface breaks up the atoms of the metal, the breakage being shown by the liberation of electrons. The energy required to liberate an electron from an atom of any given metal is known from other sources—this corresponds to the mesh of the net; in addition to this, the electron brings away with it a certain amount of kinetic energy. On adding this to the energy required to liberate the electron we invariably obtain one quantum of energy of the frequency of the incident light. If the light is of frequency such that one quantum is less than the energy required to liberate an electron we may allow the light to fall on the metallic surface for years, and no atoms are broken up, no matter how intense the light, while even the feeblest light, if of sufficiently high frequency, will at once start to liberate electrons.

Such phenomena as the photo-electric effect and the line-spectra of the elements are quite inexplicable in terms of the old dynamics, while they receive such complete and convincing explanations in terms of the quantum-theory that we might be inclined to jump to the conclusion that the quantum-theory contained the whole truth and nothing but the truth. On the other hand, all interference-phenomena and phenomena of reflection, polarisation, etc., receive a simple explanation in terms of the old dynamics, and seem at present inexplicable in terms of the new. It seems impossible to reconcile the new quantum-theory with the old undulatory theory of light. There is a very real difficulty here; indeed, it constitutes the big outstanding puzzle of present-day physics. The evidence of interference suggests that light must be continuous and almost infinitely divisible, while the evidence of the photo-electric effect is that light consists of discrete "quanta" which are discontinuous and completely indivisible.

No solution has yet been found; one has hardly been suggested. It may perhaps be worthy of notice that the theory of relativity is also to some extent antagonistic to the undulatory theory of light, for when the medium through which the waves were supposed to be propagated is abolished, the waves themselves reduce to little more than a mathematical fiction, and the undulatory theory of light reduces to the solution of a differential equation. In the past the waves in the ether have been regarded as the ultimate reality, while the differential equation has been regarded merely as a means of calculating the motion of the wave. Perhaps the scientist of the future will regard the differential equation as the ultimate reality, while the whole mechanism of the undulatory theory—ether, forces, waves, interference, etc.—will be regarded as an extraordinarily cumbersome nineteenth century model to represent the phenomena demanded by the differential equation ; the differential equation will be regarded as determining the changes of the inanimate parts of the universe except under certain conditions, which we speak of as the presence of matter, where the differential equation will yield to an equation of finite differences—the expression of the complete quantum-theory. These speculative reflections may indicate the direction in which a solution of the difficulty may perhaps ultimately be found, but they are very far from supplying this solution.

[J. H. J.]

GENERAL MONTHLY MEETING,

Monday, April 2, 1917.

CHARLES HAWKSLEY, Esq., Vice-President, in the Chair.

Murray Stewart

was elected a Member.

The Secretary announced the decease of Professor Jean Gaston Darboux, Sc.D. (Cantab), Hon. F.R.S. Hon. M.R.I., in February 1917 ; and the following Resolution passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution, and the World of Science, by the decease of Professor Jean Gaston Darboux, Sc.D. (Cantab), Hon. F.R.S. Hon. M.R.I., Secrétaire Pérpétuel de l'Académie des Sciences de Paris, Grand Officier de la Légion d'Honneur, Palais de l'Institut, Paris. He was the author of the following treatises :—" Sur une Classe remarquable des Courbes et des Surfaces Algébriques " ; and " Théorie Générale des Surfaces." His originality and distinction was such that he was honoured by the majority of the great Universities and the principal Scientific Societies of the world. He was awarded the Sylvester Medal by the Royal Society in the year 1916.

RESOLVED, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Memoirs of the Department of Agriculture: Botanical Series, Vol. VIII. Nos. 5–6; Chemical Series, Vol. V. No. 1. 8vo. 1916.

Agricultural Journal, Vol. XII. Part 1. 8vo. 1917.

Agricultural Research Institute, Pusa: Bulletin, Nos. 64–66. 8vo. 1916.

Kodaikanal Observatory, Bulletin, Nos. 53–54. 1916.

Abbadia, Observatoire (The Director)—Procès Verbaux des Séances de l'Académie des Sciences depuis la fondation, Tome VI. 4to. 1915.

Accademia dei Lincei, Roma—Atti, Serie Quinta, Rendiconti: Classe de Scienze Fisiche, Mathematiche e Naturali, Vol. XXVI. 1º Semestre, Fasc. 4–5. 8vo. 1917.

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Asiatic Society, Royal—Journal for April 1917. 8vo.

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Chemical Society—Journal for Feb.–April 1917.

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Consolo Enrico (Banca Commerciale Italiana)—The War in Italy. Vols. IV.–VI. 4to. 1916

Editors—American Journal of Science for March 1917. 8vo.

Athenæum for Feb.–April 1917. 4to.

Author for March–April 1917. 8vo.

Chemical News for March–April 1917. 4to.

Chemist and Druggist for March–April 1917. 8vo.

Church Gazette for March–April 1917. 8vo.

Concrete for March–April 1917. 8vo.

Dyer and Calico Printer for March–April 1917. 4to.

Electrical Engineering for March–April 1917. 4to.

Editors—continued
Electrical Industries for March-April 1917. 4to.
Electrical Review for March-April 1917. 4to.
Electrical Times for March-April 1917. 4to.
Electricity for March-April 1917. 8vo.
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Il Nuovo Cimento for July-Aug. 1916. 8vo.
Illuminating Engineer for Feb.-March 1917. 8vo.
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Nature for March-April 1917. 4to.
New Church Magazine for March-April 1917. 8vo.
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Weekly Weather Reports for Feb. and April 1917. 4to.
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Geophysical Journal for May-June 1916. 4to.
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Montpellier Académie des Sciences—Bulletin, Jan. 1917, No. 1. 8vo.
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Paper, No. 70. 8vo. 1917.
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Rome, Ministry of Public Works—Giornale del Genio Civile for Dec. 1916-Jan. 1917. 8vo.
Royal Colonial Institute—United Empire, Vol. VIII. Nos. 3-4. 8vo. 1917.
Royal Engineers' Institute—Journal, Vol. XXV. Nos. 4-5. 8vo. 1917.
Royal Microscopical Society—Journal, Feb. 1917, Part 1. 8vo.
Royal Society of Arts—Journal for March-April 1917. 8vo.
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Washington, National Academy of Sciences—Proceedings, Vol. III. Nos. 2-4. 8vo. 1917.
Western Australia, Agent-General of—Statistical Abstracts, March-July 1916. 4to.
Western Society of Engineers—Journal, Vol. XXI. No. 748. 8vo. 1916.
Zoological Society of London—Reports of Council for Year 1916. 8vo. 1917.

WEEKLY EVENING MEETING,

Friday, April 20, 1917.

Sir William Phipson Beale, Bart., K.C. M.P.,
Vice-President, in the Chair.

Professor R. H. Biffen, M.A. F.R.S.

The Future of Wheat Growing in England.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, April 27, 1917.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

DUNDAS GRANT, M.D. M.A. F.R.C.S. *M.R.I.*

The Organs of Hearing in Relation to War.

WHILE even a slight dulness of hearing has an undoubted disturbing effect in the simpler evolutions of a volunteer, the necessity for a reasonably good *amount of hearing-power* by the soldier under training or in the field is unquestionable. Hearing for the whispered voice at fifteen feet with either ear may be accepted as a minimum standard. Under a voluntary system it has apparently been necessary to admit men whose hearing-power was considerably below this, but where universal service affords a supply of men greater than is required such a defect would justify refusal.

Whether *deafness of one ear* renders a man unfit for service is a moot point, as one good ear is perhaps better than two poor ones. [The lecturer cited two cases of men with one-sided deafness who had proved very useful soldiers during the earlier years of the war. One had become a sergeant in a Highland regiment, the other was a driver in the Field Artillery. In neither was the one-sided deafness discovered until the other ear was temporarily deafened by shell-concussion.] A man with only one hearing-ear is not well fitted for the infantry, but may be quite useful in the artillery, transport or medical service.

There is no doubt that our pension list will be enlarged by the inclusion of *men who were passed into the army while already dull of hearing*, or affected with diseases of the ear, and whose dulness or disease has been aggravated by military service. The advisability of being able to pick and choose is an argument for the institution of a reasonable measure of universal liability to military service.

In *the Air Service* the supply has been greater than the demand, and the quality of the highest. Rigid tests of the hearing, vision and other functions are applied before candidates are admitted, and the brilliant results obtained have surpassed all expectation.

To explain the various modes in which the organs of hearing may suffer in warfare, *their structure and functions* were shortly considered in order, viz. the auditory cortical centres on the surface of the brain, the nerve tracts in the brain leading from the internal ears to these cortical centres, the internal ear itself, and the tympanic mechanism by which the sonorous vibrations are received.

A sensation of sound is appreciated by the brain, and the parts of the cortex allocated for this function have been specified by experiment and by observation of disease. By kind permission of Colonel Mott the lecturer was able to show on the screen the photograph of a brain in which the *auditory cortical centres* were destroyed by disease on the two sides successively.

The case, briefly stated, was that of a woman in whom, owing to disease—blocking of certain arteries—the auditory centre of the left side was destroyed. The result was not loss of hearing but loss of the power of understanding words and of naming things. The corresponding area on the other side gradually took on these functions and she recovered the understanding for words; she got married and the birth of her first child was unfortunately followed by a blocking of vessels on the right side; the destruction of the two auditory centres led to complete deafness and she ultimately died. Colonel Mott was able to obtain possession of this instructive specimen, and his description of it is now a "classic" in neurology.

Certain *tracts of nervous tissues* can be traced from the auditory nerve-endings in each internal ear running to *both* cortical centres. This is why destruction of one centre alone is not sufficient to cause deafness.

The *auditory nerves* themselves take their rise in certain sensory nerve-cells in *the cochlea of the internal ear*, and these cells are bathed by a liquid which is set in motion by the stapes, the innermost of the chain of bones attached to the tympanic membrane, whenever this membrane moves. The sensory nerve-cells are furnished with certain hairs which are disturbed or displaced by each vibration of the liquid (endolymph) in which they stand.* The auditory cells rest on a membrane made up of transverse fibres which are longest at the upper part of the cochlea and shortest at the base. Helmholtz taught that the fibres corresponded to particular tones, and that each entered into sympathetic vibration when the appropriate tone was sounded, very much as when we utter a sound in front of an open pianoforte with the dampers raised by the "loud" pedal.

The organs of hearing are exposed to *injury from various causes*:—

First, and very obviously, *direct mechanical damage* by bullets, fragments of shells or other missiles actually striking the hearing passages, the middle or internal ear, or possibly the nervous strands connecting the internal ear with the brain. The active structures in the internal ear can be completely disorganised by a missile such as a bullet passing through the head near the internal ear without actually touching it. This event is also observed in the retina of the eye. It is known as "commotion" or concussion, and when affecting the hearing produces what the French term "*surdité à distance.*" [The damage to the structures of the internal ear by direct violence was illustrated by micro-photographs of sections made by Dr. J. Fraser, of Edinburgh. Radiograms were thrown on the screen showing the appearance of a normal temporal bone, a temporal bone (petrous portion) rendered indistinct by the presence of the products of contusion and inflammation, and another with a fracture of the skull passing through the internal ear.]

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* In a work recently published by Sir Thomas Wrightson since this lecture was delivered, the mode of this vibration is minutely studied, and his views receive confirmation from Prof. Arthur Keith's anatomical investigations.

Second.—The force of the *compression of the air*, such as is induced by the explosion of a shell, frequently causes deafness by driving in the tympanic membrane and, with it, the small bones of the ear so as to induce a violent concussion of the internal ear. This appears to act most severely in those in whom there has been previous disease of the middle ear itself, or of the Eustachian tube and nasal passages. (Such diseases unless cured would exclude the candidate from the Flying Corps.) The explosion may at the same time rupture the tympanic membrane, and in this case some of its force is spent before it reaches the inner ear. It is found that cases of deafness due to concussion of the labyrinth are more likely to recover if there is concomitant rupture of the tympanic membrane. [The physical conditions accounting for this were illustrated by an experiment devised for the purpose. A light wooden hoop covered with a fairly resistant paper was hung by a hinge over the end of a drain-pipe. A revolver with blank cartridge was fired into the other end of the pipe and the concussion forced the hoop upwards on its hinge with great violence. The firm paper was replaced by a very thin one, and when the revolver was again discharged the paper gave way, while the movement of the hoop was exceedingly slight.]

Third.—The mere *loudness of the noises*, either one definite exposure or a continuation of such, may produce deafness by over-stimulation of the auditory fibres (or nerve-centres). Castex has shown the damaging effect of continued exposure to the sound of gunfire on guinea-pigs. Yoshii, Siebenmann and Wittmaack have exposed guinea-pigs to continuous sounds of different pitch. Patches of degeneration of the auditory cells were found in the cochlea, nearer its apex in those exposed to low-pitched tones, and nearer the base in those exposed to the higher-pitched ones. [The lecturer expressed regret that we had to depend on foreign experimenters for such important investigations. With us research was very poorly paid, either in money or in social distinction, and the teacher or original investigator had usually to support himself by the practice of medicine. Furthermore, we were hampered by unreasonable restrictions in the pursuit of truth in physiology by experiments on lower animals, desirable as it was that unnecessary pain should be avoided, and that unnecessary repetitions of experiments should be prevented. He recommended for support the society presided over by Lord Lamington, and expressed a hope that the State or the possessors of wealth would provide for the endowment of research.]

Fourth.—The auditory centres may be thrown out of gear by a *nervous shock*, the most typical of which is that of being "buried." Many who have undergone this terrifying experience have been deaf and voiceless when dug out. Slighter forms of fright produce this effect in more sensitive subjects. Such loss of hearing unconnected with physical damage to the auditory organs is termed "*functional*" *deafness*. The higher centres are, as it were, switched off, so that the sufferer is as unconscious of the sensations of sounds as if his internal ear-apparatus was destroyed. The internal ear and auditory nerves are, however, still in working order, and may be stimulated by sounds. This is shown by the occurrence of certain natural movements on exposure to sound even though the subject is quite unconscious of them. These movements are "reflex," and their occurrence indicates that the accompanying deafness is "functional" rather than physical or "organic." Recovery is usual, and is sometimes quite dramatic in its suddenness, though when and how the recovery is to be brought about it is scarcely possible to predict. As a shock brings it on, sometimes another shock removes it. In one case a fall out of bed led to complete recovery from total deafness of this form. Fright can produce degenerative changes in nerve-cells (disappearance of Nissl bodies, etc.), as shown in the special (Purkinje's) cells of the cerebellum in Crile's experiments on rabbits in relation to shock. There is also good reason to suppose that the dendrites of the neurons may retract as the result of shock and cause interruption of continuity in the nerve strands leading to the cortex of the brain. Colonel Mott favours the latter view, and in support of its feasibleness quotes Ross Harrison's observations on the growth of nerve processes from cells in the embryo.

This deafness is analogous to that which may be induced by the so-called mesmerist or hypnotist by means of "suggestion," and is no doubt identical with deafness or other loss of sensation observed in true hysteria. The "conscious" is absent, but the "sub-conscious" is still more or less to the fore and open to impressions which the "conscious" would rule out or inhibit when not stupefied by fear or terror.

Fifth.—*Destruction of both auditory cortical centres* as the result of wounds would produce deafness, but, of course, such an extensive injury could scarcely be compatible with life.

Cases frequently occur in which more than one of these factors are present, and we may find associated with "functional" deafness affecting both centres an organic damage to one of the labyrinths.

Sixth.—Apart from injury, *diseases of the ear* are the cause of many of the cases of war-deafness. Inflammatory conditions of the ear frequently arise as the result of exposure to cold or damp or infectious disease of various kinds, including mumps and epidemic cerebro-spinal meningitis, popularly known as spotted fever. The latter is the cause of some of the most deplorable cases.

The *hearing-power in general is tested* by means of the voice, the watch, and numerous other sources of sound, but the extent of hearing for tones of various pitch is usually measured by means of tuning-forks. [The lecturer pointed to a fallacy in the use of tuning-forks, viz. that the hearing-power is usually calculated as in direct proportion to the length of time the tuning-fork is heard. In reality the tuning-fork dies away in proportion to the logarithm of the time, i.e. the Naperian logarithm. The lecturer had dealt with this question in a paper read before the International Otological Congress held at Boston, U.S.A., in 1913.]

For the highest pitched tones the hearing was tested by means of Galton's Whistle, the steel wire or monochord, or small steel rods. Retention of hearing for these tones indicates integrity of the most vulnerable portion of the cochlea (i.e. the portion nearest the base, with the shortest fibres), and hence greater probability of recovery.

The *equilibrial portion of the labyrinth* consists mainly of the semicircular canals forming the posterior half of that organ. [Their structure was shown by large diagrams and their small size by the actual human skull.] They lie in three planes, so that they are displaced by movement of the head in any direction whatever. Inside their membranous lining there is a fluid which lags or continues in movement when the canal is moved or when it stops, thus disturbing the hairs on certain minute cells in the dilated portions of the tubes. These hair-bearing cells are connected by nerves with certain nuclei and with the cerebellum and spinal cord. They have also important communications with the nuclei of the nerves which control the movements of the eyeballs. The eyeballs enter into appropriate movements when the position of the head changes, and this is induced by the disturbance set up in the semicircular canals. If we rotate a person with normal semicircular canals the eyes make definite jerking movements, and if these movements do not take place we assume that the semicircular canals are damaged. If again we apply cold to the labyrinth, downward currents are produced and jerkings of the eyeballs follow. [The lecturer illustrated the effects of rotation by means of a circular trough filled with water in which stood some flexible reeds. He also explained the formation of downward convection currents in a large beaker of water, on the surface of which a lump of ice was placed. Upward currents were shown by means of a spirit-lamp below a beaker of water.]

The "walking-stick" test introduced by Prof. Moure, of Bordeaux, is carried out as follows: The subject bends down, rests his forehead on the top of a walking-stick and walks round the stick several times. He is then suddenly ordered to stand up and walk straight forward. If his labyrinths are normal he cannot do this, and he reels round to the side towards which he has been turning. [This was demonstrated by the lecturer.] If the labyrinths are destroyed, the subject walks straight forward in spite of the rotation. [A soldier whose labyrinths had both been destroyed by specific disease was shown and submitted to this test at the end of the lecture. He was seen to walk straight forward without difficulty.]

The integrity of the equilibrial labyrinth can thus be tested by rotation or by syringing cold or hot water into the ear. The most convenient means is the blowing in of cold air by means of the lecturer's *cold-air labyrinth-testing apparatus*, as used in the French army.

These tests are important because they indicate the condition of the semicircular canals. If then along with deafness there is evidence of damage at the same time to the semicircular canals, the great probability is that the destruction of the internal ear has been very considerable, that the lesion is a severe one and unlikely to be followed by recovery.

Simulation of deafness for the purpose of escaping dangerous duty or of securing compensation must be very tempting. It is not surprising that it should occur, but it is very surprising (and creditable) that it should occur so seldom. There are tests by which it can generally be detected, but their consideration here would "not be in the public interest."

The *frequency* with which deafness, either temporary or permanent, arises in the course of active military service in the field can be only approximately surmised. In a number of our military hospitals about 1·4 per cent suffered from some form of deafness. In two German Army Corps 2013 cases of disease or injury of the organs of hearing occurred in four months. It is to be remembered that many of the cases were those of ordinary inflammation of the middle ear, apart from injury, incidental to general disease or exposure to cold or wet, and that in many the ear-disease was existent, though perhaps quiescent, before enlistment. Professor Moure (Bordeaux) and Professor Lannois (Lyons) have given interesting statistics as to the nature of the disease or injury. The latter gives the results of his observations and treatment in 1000 cases under his care between March and December, 1915, as follows:—

1. Cases with existent ear-disease ... ... (189 had suppuration, 134 had not.)	323
2. Cases without existing ear-disease ... ... (262 simple concussion of labyrinth; 383 concussion of labyrinth with rupture of membrane, in which 301 suppurated and 82 did not.)	645
3. Cases of deafness from "shell-shock"	32

He found that in the absence of direct injury labyrinthine concussion was rarely followed by lasting deafness, and that deafness from concussion was much more frequent in those who had pre-existing ear-disease than in those whose ears were previously normal.

The *results of treatment* were in proportion. Of those with previous disease 50 per cent were able to return to full duty, while of those with previously healthy ears all but 5 per cent recovered in whom the drum was not ruptured, while all recovered in whom the drum had given way. Early and appropriate treatment is obviously of the greatest importance both from the physical and the moral points of view.

The *prevention of war-deafness* is a question associated with that of ear-protectors. These were dispensed with as a rule, or after use were usually discontinued. A number of officers have, however, borne testimony to the effectiveness of the Mallock-Armstrong protector in deadening the effect of explosions, while permitting of the hearing of orders. Lake, Jenkins, and others have devised ingenious plugs for the ears, and of course cotton-wool and plasticine have also found favour. The objection to many of these is the risk of their being driven in. Cases have occurred in which a plug of ear-wax in the ear has appeared to have protected the internal ear from damage by explosion, although of course the driving in of the plug of wax has caused temporary complete deafness, hearing being restored on removal of the wax. *Habituation* to the noise was usually established after a time, the subjects apparently acquiring a power of discrimination and of attending to or disregarding noises. An officer stated that during the continual rumbling din of a bombardment the report of a heavy gun close at hand was scarcely observed, while away from such bombardment the firing of a single small gun was startling enough to make the hearer jump. [The lecturer demonstrated this by having a rumbling roll on two kettledrums played crescendo. Loud bangs on a big drum given at irregular intervals were scarcely perceived as anything more than a dull thud. When the kettledrums were silenced the bangs on the big drum were almost ear-splitting.]

The *after-condition of the deaf soldier* depended on the degree of his deafness. If moderate it might not prevent the continuance of occupation in many trades or businesses, though in others it might be absolutely prohibitive. In very many it was a more serious handicap, such as the business of barristers, salesmen, canvassers, booking-clerks, and in general any business calling for rapid oral communication or comprehension of orders or instructions.

A deaf writer has expressed the opinion that the dull of hearing are apt to forget the amount of trouble their affliction causes to other people. Hence employers are chary of employing them, not so much on account of their deafness as of the favours and privileges they demand. In them early appropriate treatment was essential and often beneficial.

For the extremely deaf, as such, many occupations were completely barred, but "lip-reading" often came to their assistance, and in suitable cases almost made up for loss of hearing. It is to be remembered that absolute deafness is sometimes purely functional and open to the possibility of complete recovery.

Among the *occupations* followed by actual deaf-mutes have been noted boot-, shoe- and clog-making, labouring (of various kinds), tailoring, metal and machine-working, agricultural labour, carpentering and joinering, cabinet-making, printing, French-polishing, painting and decorating, basket-making, saddlery and harness-making, baking, etc. There is then still more scope for the simply deaf if given the opportunity for training.

The *teaching of "lip-reading" to deafened men* is quite different from teaching speech and lip-reading to deaf-mutes who have no basal knowledge of speech. In these the lip-pictures corresponding to the elementary sounds of which speech are made up have to be laboriously presented, whereas for the deafened man a more synthetic method is practised, that is, exercising him in the recognition of words, phrases and sentences, rather than simple sounds, by the movements of the lips and face.

The time required depends on the capacity of the individual, good sight and general alertness being important factors. Sometimes the more highly-educated subjects have greater difficulty than the less cultivated in acquiring the knack of rapidly grasping the speaker's meaning. It must of course be recollected that the learned man has a much larger accumulation of words and ideas among which to pick out those intended by the speaker, and he has, therefore, more hesitation in committing himself. In three months a fair proficiency is expected and very frequently attained. German authorities allow five or six months. The French seem to acquire the art with great facility, and the French mode of utterance is probably more favourable for lip-reading than the English or German.

It has been suggested that in view of the obvious limitations of lip-reading, deaf men might be taught *finger-spelling*. This would, of course, put the deaf man in communication only with the comparatively few people who speak on their fingers, and with them he would be well enough off with a pencil and writing-tablet. It would not put him in touch with the world at large, or even enable him to read the word "Daddy" on his little child's lips. Lip-reading re-opens the world to the deafened man in a way only surpassed by restoration of hearing-power, which should, of course, receive the first consideration.

Compensation in the form of *pensions or gratuities* to those deafened through military service should be in proportion to the degree to which the subject's earning-power is diminished, and this is calculated mainly on the degree of deafness. Complete deafness ranks as the equivalent of the loss of one limb for pensioning purposes. In our warrant it counts as 70 per cent of complete disablement, and carries a minimum pension of nineteen shillings and sixpence a week.

The assessment of the degree of dulness of hearing calls for tact, judgment and sometimes ingenuity on the part of the examiner.

Although the case of the deaf man is not so hard from the wage-earning point of view as that of the blind one, it has *some peculiarly pitiable aspects*, and is often characterised by a degree of moroseness, suspicion and depression from which the sightless soldier is singularly free. Those who may understand his difficulties and needs, and who may be in a position to help in removing them, would be doing a truly humane act by taking up his case.

Postscript.—The lecturer has (since the delivery of this lecture) had the opportunity of visiting various lip-reading classes for soldiers in France, and of reporting on them. As President of the Special Aural Board under the Ministry of Pensions, he has taken a share in the establishment of lip-reading classes in London and the various "pension areas" throughout the kingdom. (The earliest established class was, however, the one conducted in Edinburgh by Miss Stormonth.) The headquarters in London are located at 28 Park Crescent, where lip-reading classes are carried on daily, while recreation and refreshment are also provided. In this way the fatigue incident to the strain of attention during the class-hours is neutralised to the utmost possible extent. Reports on the subject are found in "Recalled to Life," No. 3, and in the Reports on the Inter-Allied Conference on the "Treatment and Training of the Disabled," London, 1918.

[D. G.]

ANNUAL MEETING,

Tuesday, May 1, 1917.

THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S., President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1916, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Thirty-three new Members were elected in 1916.

Sixty-two Lectures and Nineteen Evening Discourses were delivered in 1916.

The Books and Pamphlets presented in 1916 amounted to about 316 volumes, making, with 540 volumes (including Periodicals bound) purchased by the Managers, a total of 856 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.
TREASURER—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.
SECRETARY—Colonel E. H. Hills, C.M.G. R.E. D.Sc. F.R.S.

MANAGERS.

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Sir William Phipson Beale, Bart., K.C. M.P.
Charles Vernon Boys, F.R.S. A.R.S.M.
J. H. Balfour Browne, K.C. LL.D.
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The Rt. Hon. Viscount Iveagh, K.P. G.C.V.O. LL.D. F.R.S.
Sir Charles Nicholson, Bart., M.P. M.A. LL.B.
The Hon. Richard Clere Parsons, M.A. M.Inst.C.E.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
Alexander Siemens, M.Inst.C.E. M.I.E.E.
Samuel West, M.D. M.A. F.R.C.P.
The Rt. Hon. Lord Wrenbury, P.C. M.A.

VISITORS.

Ernest Clarke, M.D. B.S. F.R.C.S.
John F. Deacon, M.A.
Edward Dent, M.A.
Lieut.-Colonel Henry E. Gaulter, F.R.G.S.
W. B. Gibbs, F.R.A.S.
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W. A. T. Hallowes, M.A.
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H. R. Kempe, M.Inst.C.E.
Francis Legge, F.S.A.
James Love, F.R.A.S. F.G.S.
Richard Pearce, F.G.S.
Sir Alexander Pedler, C.I.E. F.R.S.
Hugh Munro Ross, B.A.
Joseph Shaw, K.C.

WEEKLY EVENING MEETING,

Friday, May 4, 1917.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

H. Wickham Steed, Author of "L'Angleterre et la Guerre."

Some Guarantees of Liberty.

It is rather more than three years since I had the honour, at the end of January, 1914, to address you upon "The Foundations of Diplomacy." At the beginning of that discourse I gave, as an explanation of its genesis, the gist of some correspondence which had passed between me at Vienna and an influential Englishman in London in the autumn of 1912, in which I had insisted that complications between Austria and Serbia might entail a European conflagration, and compel Great Britain to fight at a moment's notice for the defence of the Scheldt and even of Calais, that is to say, of the "Narrow Seas," against a German army. In addressing you, I deplored the ignorance in which our people had been left as to the essential conditions of our national freedom, and asked, "Why should the British Government be deprived of the strength that comes from the support of an awakened and well-informed public opinion? Why should our diplomatic action be hampered by the inability of our people to understand the bearings of issues that may drag us, willy-nilly, into a life-and-death struggle, whereas appreciation of the dangers involved might have enabled us to exercise our influence discerningly, and in time?"

I defined the true basis of diplomacy as "living knowledge in the service of an ideal," and said that, "if a nation is to play the part of a living, driving force in the world, it must have a conscious ideal. This ideal must be so plain and clear, so evidently connected with the higher interests of the community, that it shall commend itself to and command the instantaneous support of the majority of right-feeling citizens." In conclusion, I declared it to be my profound "conviction that no shrewd calculation of interests, no canny avoidance of moral responsibilities, avails to replace a sane ideal in the management of foreign affairs; and that if our nation is to come safely through the trials that may be in store for it, our people must again be taught a sound ideal—not, indeed, an ideal divorced from reason, but such as to inspire it and those who control its affairs with the belief that the thing chiefly needful is to know what is just and right, and to be ready and able to do it because it is just and right." President Wilson has recently packed the whole of my contention into five pregnant words: "Right is better than peace."

Neither I, nor any man, save possibly a small group of German soldiers, statesmen and bankers, could know in January, 1914, that in six months' time the Great War would be upon us. But those who, like me, had lived for more than twenty-one years the political life of great continental States, felt that matters were slowly coming to a head, and that, in view of the deliberate purpose of Germany to secure the mastery of Europe and of the world, the date of the inevitable conflict would depend upon the readiness of liberty-loving States to defend their freedom against pretentions that she was certain to put forward at what might seem to her the most propitious moment. My desire was to insist upon the need for public knowledge of the danger to which our liberties were exposed, and to make it clear that it is as "worth while" to fight in defence of our ideals as it is to concentrate diplomatic or national endeavour upon the "practical" objects of securing wealth and promoting trade. I added that "the doctrines known as 'pacifism,' and all cognate apologies for national unreadiness based upon faith in the pure intentions of others, are, I believe, the surest pledge of disaster." and that "the problem we have to consider is what constitutes in the modern world, effective force—to what extent physical power is valueless without the co-efficient of moral strength used in the light of knowledge."

The lessons of the war have to some extent given us the answer to that problem. The war has shown that, given equality of armament, it is still moral strength that prevails—nay, that on occasion moral strength may more than counterbalance superiority of hostile armament. We have seen that if the Allied Powers can now hope to prevail against the iniquitous force of the enemy, it is because the moral quality of their cause will have enabled them to bring to bear still greater force in the service of right and in the defence of human freedom.

What, however, do we mean by "freedom"? Before we can hope to safeguard it we must know what it is we wish to safeguard. For our present purpose the idea of freedom falls practically into two categories—the freedom of the individual in the community and the freedom of the community itself. These are inter-related and inter-dependent. With the kind of freedom which an isolated individual might possess in an otherwise uninhabited island we have no concern. To him, his every whim would be law, his only care the thought of his own preservation. He would be non-political and non-moral because divorced from and outside any community. The freedom we have to consider is therefore the freedom of the individual in the community, together with his preservation as an efficient unit in the community, and the preservation of the freedom and independence of the community as an efficient unit in the society of free nations.

The war has rendered us no greater service than that of reminding us that individual freedom is contingent upon the preservation of the independence of the community. It has taught us that individual liberties preserved or acquired during centuries of political struggle must be surrendered when the life of the community is at stake. Save among small minorities of extreme pacifists and "conscientious objectors," this truth is to-day unchallenged. It is, moreover, sound liberal doctrine. John Stuart Mill recognized it in the Introduction to his essay on "Liberty." "There are also," he wrote, "many positive acts for the benefit of others which he (the individual) may rightfully be compelled to perform; such as to give evidence in a court of justice; to bear his fair share in the common defence, or in any other joint work necessary to the interest of the society of which he enjoys the protection; and to peform certain acts of individual beneficence, such as saving a fellow-creature's life, or interposing to protect the defenceless against ill-usage, things which, whenever it is obviously a man's duty to do, he may rightfully be made responsible to society for not doing. A person may cause evil to others not only by his actions but by his inaction, and in either case he is justly accountable to them for the injury. The latter case, it is true, requires a much more cautious exercise of compulsion than the former. To make anyone answerable for not preventing evil is, comparatively speaking, the exception. Yet there are many cases clear enough and grave enough to justify that exception."

This doctrine Mill held to be entirely compatible with the maintenance of "absolute freedom of opinion and sentiment on all subjects, practical or speculative, scientific, moral or theological." War for the defence of the community, or preparation for the eventual defence of the community, are certainly contingencies "clear enough and grave enough" to justify compulsion; and the application of compulsion cannot in these cases be held an infraction of the rightful freedom of the individual.

The fundamental characteristic of a liberal conception of freedom is that it regards compulsory interference with the life and conduct of individuals as the exception, not as the rule; and looks upon them rather as necessary qualifications of a general principle than as ends in themselves. Since the applications of compulsion and the restrictions of individual liberty have to be carried out by the representative organs of the community—that is to say, by the State—it is evident that the liberal conception of freedom implies a liberal conception of the nature and of the functions of the State itself.

Here we come to the root of the difference between the British, or rather the Anglo-Saxon, and the German conceptions of individual freedom. This difference has sometimes been indicated by the saying that, in Anglo-Saxon communities, everything is permitted that is not expressly forbidden ; whereas in Germany, everything is forbidden that is not expressly permitted. Fears have been expressed lest the growing organization and militarization of the community under stress of war, end by imposing upon us the very evil of Prussian militarism which we and our Allies are pledged to destroy. It cannot be gainsaid that there is some danger that extreme organization for purposes of defence may tend to crystallize the wholesome fluidity of our ideas and institutions, and to leave the body-politic afflicted by arterio-sclerosis. It is not that we are ever likely to contract the Prussian form of the militarist disease, but, unless we are careful, we may develop a malady of our own of which the effects would be singularly pernicious. Thoughtful French writers have defined this war as a conflict between irreconcilable sets of ideas. It certainly reveals, if it is not the direct outcome of, a fundamental difference between Central and Western European conceptions of the State and its functions. What may be called the liberal Anglo-Saxon view of the State is that, in normal times, its functions should be limited to a minimum indispensable to the conduct of public affairs, and that it should interfere with the life and liberties of individuals only in so far as such interference may be required in the interests of the community as a whole. Government is thus regarded as a necessity imposed by the exigencies of public order and national defence, not as an end in itself. The Government, which is the active expression of the State, therefore stands as the servant of the community, and the servants of the State are, in theory if not always in practice, the servants of the community. The German, or at least the Prussian, theory is, on the contrary, that the State is something apart from and higher than the community. The members of the community are the servants of the State. It follows that the officers of the State are not only in practice but in theory the masters of the community, possessing attributes superior to those of the private citizen because they are derived from the State, of which the head is a Monarch governing by divine right and therefore answerable in the last resort to the Creator alone. The political philosophy of Germany, especially since Hegel, teems with mystical glorifications of the State. Treitschke positively deifies the State. In his *Politik* he refers to "the objectively-revealed Will of God as unfolded in the life of the State"; and calls the State "the most supremely real person, in the literal sense of the word, that exists." And again, "the State is the highest community existing in exterior human life, and therefore the duty of self-effacement cannot apply to it. As nothing in the world's history is its superior, the Christian obligation of sacrifice for a higher object is not imposed (upon it)." In the course of our history we, too, have known and resisted, not without success, the doctrines of divine right and the tyranny of Kings and State officials claiming to possess a semi-transcendental authority deriving from their position as delegates of royalty; but we have fortunately never had a Hegel or a Treitschke to frame for the British State a pseudo-scientific character of absolutism. In any case our national sense of humour would probably have saved us from taking seriously the pretentious phrase-making of a philosopher like Hegel, which the keen critical sense of Schopenhauer, in his *Kritik der Kantischen Philosophie*, castigated as follows:—"The greatest impudence in dishing up pure nonsense, in pasting together wild and senseless tissues of words, such as have hitherto been found only in lunatic asylums, appeared in Hegel and became the instrument of the grossest general mystification ever seen—with a success that will seem fabulous to posterity and will remain a monument of German foolishness." Nevertheless we have not entirely escaped the contagion of Hegel's "German foolishness." Some of our writers and political thinkers have mistaken it for transcendental wisdom. The effects of their advocacy of German ideas are clearly to be discerned. We need therefore to be on our guard lest we go astray when we presently find ourselves confronted by unforeseen consequences of the great sacrifices of individual liberty which we have made—and rightly made—during the war in the cause of national self-preservation.

These sacrifices have been prompted by the sound and healthy instinct that recognizes the sense of responsibility towards the community on the part of the individual to be an indispensable corollary of individual freedom in the community. This sense of responsibility is the very basis of freedom. All changes in State reorganization that tend to shift responsibility from the individual on to the State and to make individuals feel that others are watching over and for them, need to be accompanied by a corresponding increase in the moral and political virility of the individual. Otherwise the weakening of his sense of personal responsibility would tend to diminish the security of the community as a whole.

There is some analogy between the true doctrine of individual freedom in a free community and that of the Roman Church in regard to the "rights" of its members. Some twenty years ago an enterprising Roman prelate sought to prove that the doctrine of his Church was entirely compatible with the passage in the American Constitution which declares every man to have a natural right to "life, liberty and the pursuit of happiness." He was presently admonished by the ecclesiastical authorities and instructed that the only "rights" possessed by Christians are contingent upon the performance of their duty—the duty of saving their souls. Though the doctrine of civil liberty in an organized community cannot be thus dogmatically stated, and though the work of a free community is to ensure, in normal times, a maximum of ordered freedom to its members, it is clear that, at moments of crisis and common danger, questions of individual freedom lose significance in comparison with the paramount necessity of preserving the community itself. The real problem to be solved is how to ensure a maximum of individual freedom in normal times with such efficiency of cohesion and spontaneity of discipline in times of stress, as to safeguard the community against attack from outside or unwholesome tendencies within. In this sense it may be said that, according to sound liberal doctrine, the individual citizen has a right to liberty within the community not in order that he may follow his own will in every act of his life, but in order that he may use his liberty for the service of the community. He is entitled to liberty because he cannot make his full contribution to the life of the community if he be the servant of the will of others, and if he be not able to bring to the common stock the fruit of his own activity and of his own judgment. Analogies between the structure of living organisms and that of organized communities have become hackneyed and are often misleading, but it remains true that, just as the health of living tissue depends upon the health and vigour of each individual cell, so the health and power of resistance of a community may depend upon the self-reliant vigour of each of its members.

How far did the social and political conditions prevailing in Great Britain before the war fulfil the requirements of a free and healthy community? To what extent were the "free institutions," of which we had been taught to be proud, discharging their functions? I cannot presume to claim infallibility for my personal impressions, though I had been accustomed for many years to observe the conditions of foreign communities and had long watched the course of affairs in England with a fresh and impartial eye. The impression made upon me by England in the years before the war was that of a community in rapid process of political and social degeneration. Our "free institutions," built up by centuries of struggle against autocratic tendencies, seemed to have fallen into the hands of a practically irresponsible oligarchy. I am not referring to any men or party in particular, but to a phenomenon. I have never belonged to any party. My observations date at least from the beginning of the century. In theory, the people controlled the working of the State through their representatives in Parliament, a majority of whom, in their turn, delegated executive functions to a small body of Ministers. These controlled, ostensibly, the workings of departments over which they presided, and were jointly responsible to Parliament for the good government of the country. In reality, party organizations, supported by contributions from interested individuals or groups of individuals, foisted upon the people, by means of elaborate propagandist machinery, notions which it was in the interest of the party that the people should accept. Every art of presentation and misrepresentation was employed to prevent the people from forming an impartial opinion. When, in these circumstances, popular representatives had been chosen, often with the help of party funds, those representatives became the slaves of the party machine, were compelled by all kinds of pressure to forego freedom of judgment, and to vote as the party managers might require. If the heads of the party happened to be in office, they were concerned far more with keeping themselves in office than with their guardianship of the welfare of the community. No means existed of bringing them rapidly to book for mistakes or misdeeds. It became a party interest to repel any attack upon them, and they could always be sure of a packed party majority in a machine-made Parliament. Within the departments over which they presided they were in appearance supreme, but in practice their ignorance placed them at the mercy of their technical advisers, the permanent officials, who were irresponsible save to them. Upon the errors of permanent officials there was little or no check. Public criticism or attack upon those officials was resented as "bad form" since they were unable publicly to defend themselves. If attack were made upon their responsible parliamentary chiefs, the packed parliamentary majority held those chiefs harmless. Between the various departments of State there was little cohesion. England seemed to be governed by a disjointed series of administrative despotisms.

Upon this system of concatenated political and economic interests there was but one effective check—the power of publicity exercised mainly through the Press. But the bulk of the Press was also attached by a hundred ties to the party system. Newspapers appeared, moreover, more eager to maintain their circulations by tickling the ear of the public and by giving the public "what it wanted," than to court unpopularity by admonishing and educating the people. There were exceptions, but they were few.

Then came the war. The full story of the critical days that preceded the entry of Great Britain and of the British Dominions into the war cannot yet be publicly told. In consequence of our shortcomings, of our guilt in leaving the people uninstructed as to the fundamental conditions of the freedom—nay, of the very existence of the community—we found ourselves, blindfolded, on the verge of the abyss. We escaped by a miracle—a miracle not entirely of our own working. But we resolved to do our duty, come what might, and entered with stout hearts and ignorant minds upon the greatest revolution in our history.

We now know how far that revolution has hitherto brought us. We do not know how far it may still carry us. We know only that there can be no turning-back, and that we must press forward to the bitter, or, as we firmly believe, to the triumphant, end. But the end will leave us with many new problems to face, and many an unsuspected danger to our individual and national freedom. I do not refer to the adjustments that may be requisite in our system of national defence; those adjustments may be constitutional, military, naval, economic and financial. The development of the submarine and airplane; our realization of the fact that, though linked more firmly with the Oversea Dominions and, happily, in closer agreement with the United States of America, these Islands will henceforth be more than ever a part of the continent of Europe; the need for maintaining firm political and economic alliances; the recognition of the insidious character of cosmopolitan finance, and of the truth that a nation's wealth forms an integral part of its defensive resources—all these things may work radical changes in the forms of our provision for national security. But these things are, so to speak, externals. I have no fear of what is called "militarism." Militarism is, in the last resort, a psychological product, and our minds are not "made that way." But we are threatened by a far more serious danger—the danger of an overgrown, over-organized and consciously over-weening officialdom which, in the form of an immense bureaucracy, may strangle our ancient liberties with red-tape.

Before returning to England I wrote a book upon the Hapsburg Monarchy, in which some hard, though I believe true, things were said of the Austrian official class or bureaucracy. Had I then possessed as much knowledge as I have since obtained of the ways of British officialdom, some, though by no means all, of the criticism of the Austrian official world would have been qualified by an indication that in England things were not very different. Even before the war the problem of the bureaucracy was fast becoming a nightmare in Austria. More than 900,000 individuals out of a total population of some 30,000,000 were in the direct employ of the State, and were entitled to regard themselves as masters of the public. Their division into eleven ranks or classes, each with its special emoluments and privileges, had given to the Austrian State and to Austrian society a semi-Chinese character and an Asiatic inelasticity. I doubt whether, even in Prussia or in Russia, the bureaucratic problem can be so profitably studied as in Austria. Its study forced upon me the conclusion that in no community where officialdom predominates and feels that it is "the Government," can there be any effective safeguard of individual freedom.

One of the most characteristic illustrations of Austrian red tape was related to me by Count Clary und Aldringen, a former Austrian Premier. During his premiership a greengrocer in one of the Vienna thoroughfares began to sell dried vegetables. A grocer, who also sold dried vegetables and whose shop was in the same street, laid an information against him for violating the *Gewerbeordnung*, or industrial regulations. The police appointed a commission of inquiry, which decided, as a legal question was involved, to refer the matter to the Captaincy of the District. The Captain of the District appointed a commission of legal experts, who debated the question at great length and reported that the legal principle was so important that they were incompetent to give a decision. They therefore referred it to the Lord Lieutenancy of Lower Austria, an administrative department which employs several hundred legal experts. The Lord Lieutenant appointed a special commission of lawyers in his turn. They debated the question in its length and breadth for several weeks, and finally agreed that the legal issue was so grave that it must be decided by the Presidency of the Council of Ministers. The President of the Council, Count Clary, appointed yet another commission of legal experts, whose chairman eventually reported as follows to the assembled Cabinet:—"The legal principle involved is so serious that your Commission is unable to agree in regard to it. Arguments of equal weight may be adduced in support of either view. In doubt as to the right decision, I ventured to consult my wife, who said, 'Dry or fresh, vegetables are vegetables.' In this sense I beg to report to your Excellencies." This view was accepted by the Cabinet, and the greengrocer, after eight months' suspension, was allowed to sell dried vegetables!

One point upon which I cannot venture to express a definite opinion is whether the officials who man our departments of State in England yet feel that they are "the Government," or aspire deliberately and wittingly to manage public affairs according to their own ideas or interests. I do not know—notwithstanding some alarming symptoms—whether our officials, as a body, have yet acquired a corporate consciousness, whether they regard themselves as forming a State within the State, and as having a vested right in the management of public affairs. On the whole, I doubt whether they have yet reached this stage; whether the cumbrous practices which we call red-tape are more than an inherited routine developed by dislike of individual responsibility, or whether they are contrivances, deliberately and consciously maintained, for putting sand in the wheels of the State whenever those wheels threaten to revolve in other directions, or more rapidly, than officials approve. We call our officials "civil servants" of the Crown. The danger is that they may consciously become, if they are not already, civil masters of the community. In this danger may lie one of the gravest menaces to public freedom, unless adequate safeguards are sought and applied in time.

I feel no hostility towards officials either individually or as a class. The majority of them are upright, hardworking men, some of whom constantly save the community from the effects of ministerial incompetence. It may indeed be said that without permanent officials drawn from the most intelligent and best educated classes of the community, the parliamentary system would be unworkable and continuity in administrative methods impossible. These high permanent officials might often, in other circumstances, have become true statesmen, but the great influence they wield is vitiated by their lack of publicly ascertainable responsibility. It is this lack of direct responsibility, that is to say, of risk, that constitutes the danger of officialdom. In some democratic communities a remedy has been sought in the system of "recall," that is to say, in the appointment and dismissal of officials by popular vote. This system is undeniably vicious. It tends towards instability and administrative anarchy. But the other extreme of entrusting an ever-increasing volume of public power to an ever-increasing body of practically intangible officials is scarcely less dangerous.

The essence of the bureaucratic or official spirit is indeed dislike of responsibility outside the, generally narrow, limits of the official's special functions; and the exercise of absolute authority within those limits. Subservience towards superiors, out of a spirit of discipline or for the purpose of self-advancement, is apt to be accompanied by arrogance towards inferiors and towards the public. By degrees, officials tend to lose the notion of liberty. They have little themselves and can hardly be expected to be careful of the liberties of others. They are parts of a machine; and, in a machine, moral courage and individual initiative are apt to be disturbing factors. They are the victims of a system of division of labour which, while excellent and indeed indispensable in mechanical production, tends to make automata of human beings who cannot thrive and develop, morally or mentally, without a constant sense of individual risk and responsibility.

Like all rigid organizations, departments of State with their limitations of responsibility for those whom they employ, tend to generate a caste spirit which often replaces the sense of the larger duties of citizens. Often, too, acute rivalry grows up between the officials of different departments, who come to regard the object of securing advantages for their own departments as more important than the supreme object of ensuring public welfare. Their functions render them imperfect citizens and, to that extent, detract from their fitness as members of the body politic. During the war we have seen a vast and rapid increase of officialdom. The extension of State control over industry, railways, mines, shipping, canals, to say nothing of the introduction of compulsory military service and the whole work of national registration and national mobilization, implies an immense increase in the army of officials. It cannot be supposed that the new departments and sub-departments of State will disappear with the advent of peace and that things will revert automatically to the *status quo ante bellum*. These new organizations will tend to become permanent, to regard themselves as ends in themselves, to acquire a corporate consciousness, and to defend by every means in their power what will have become vested bureaucratic interests. The very numbers of the men and women they employ will make of them potential electoral *clientèles* whose aspirations and grievances will attract the demagogue. Upon the unofficial section of society will fall the burden of maintaining the legions of new "public servants" who will come more and more, unless adequate safeguards be provided, to regard themselves as masters of the public.

What are these safeguards, and where is the remedy? There is no absolute remedy, no universally effective safeguard. The quality and independence of Parliament may perhaps improve. But, in view of past experience, it would be ingenuous to suppose that Parliaments will cease to be machine-made or that the "caucus" and the electoral mechanism which it controls will not continue to eliminate the spirit of independence from among the "representatives of the people." The only true safeguard lies in the political education of the people—including that of coming generations of civil servants—and in the cultivation of a spirit of independence and economic self-reliance among individuals. The liberal doctrine of the State must be inculcated upon the young, and officials must be regarded—inasmuch as they will have sacrificed some part of their individual freedom for the sake of safe and permanent employment—as citizens belonging to a category a little lower than that of the unofficial members of the community. Cases of official arrogance, obstruction or ineptitude must be publicly exposed—but let those who expose them be prepared for the pertinacious, clannish resentment of the immediate colleagues of offenders, if not of the whole official class! The law of libel may need modification and clarification. Publicity, combined with a healthy spirit of individual independence, is perhaps the greatest safeguard—and in this respect heavy responsibility will devolve upon the Press. It is true, in a sense, that the public usually has the kind of Press it deserves; but it is important that, in a free community, the organs of public opinion should be numerous enough not to fall wholly under the control of any one party or financial interest; that they should be wealthy enough not to be entirely corrupted or "influenced," disinterested enough not to become altogether commercialized, and high-minded enough not to pander to public vices or to yield to public clamour. But, when all is said and done, the problem of preserving individual freedom in an organized community is a problem of moral and political education, a problem of knowledge of the essential conditions of freedom and of the will to use it. There is no panacea, but there is, in the education of citizens to a sense of their duties and of their rights, to a love of liberty, to a hatred of tyranny, small and great, to moral courage, truthfulness and uprightness, a very potent safeguard against the evils that may threaten us when once the menace of the outer foe shall have been overcome.

[H. W. S.]

GENERAL MONTHLY MEETING,

Monday, May 7, 1917.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc F.R.S.,
Treasurer and Vice-President, in the Chair.

Lady Bax-Ironside,
Sir J. Stirling Maxwell, Bt.,
Helena F. Mond,

were elected Members.

The Chairman read the following letter, which had been received from the Honorary Member elected at the General Meeting on December 4, 1916 :—

MINISTÈRE DE LA GUERRE,
REPUBLIQUE FRANÇAISE, PARIS :

le 2 *Mai*, 1917.

MESSIEURS,

Vous voudrez bien ne pas me tenir rigueur de l'extrême retard que j'apporte a m'acquitter envers vous.

La seule excuse que j'invoque m'est fournie par les circonstances ; au moment ou je recus notification du haut honneur que vous m'avez confére, j'exercais les fonctions de Ministre des Inventions de Guerre ; j'ai aujourd'hui la lourde charge de présider a l'organisation de notre defense nationale.

La distinction dont le suis l'objet, Messieurs, de la part de votre illustre Compagnie, m'est précieuse à bien des titres.

D'abord, parce que peu de mes compatriotes l'ont obtenue au nombre desquels fut le grand Pasteur; et c'est l'une succession dont même un membre de l'Institut de France a quelque raison de s'intimider.

Ce qui me fait, ensuite et surtout, attacher un prix particulier a l'honneur d'avoir été choisi par vous pour faire partie de votre Assemblée, c'est que ce choix s'est manifeste en pleine guerre alors que nos deux pays combattent côté-a-côté le combat du droit.

Puissent ainsi toutes les forces intellectuelles et toutes les ressources scientifiques de l'Angleterre et de la France hater la victoire de nos armes et assurer pour jamais dans le monde la suprématie de la pensée sur la violence.

Messieurs les membres de la Royal Institution, je vous prie d'agréer l'expression de ma consideration la plus distinguée et de mon dévouement fraternel.

Le Ministre de la Guerre, Membre de l'Institut,

PAUL PAINLEVÉ.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Annual Report on Kodaikanal and Madras Observatories for 1916. 4to.
Report on Progress of Agriculture in India for 1915-16. 8vo. 1917.
Accademia dei Lincei, Reale, Roma—Rendiconti, Classe di Scienze Fisiche, Mathematiche e Naturali. Serie Quinta, Vol. XXVI. 1° Semestre, Fasc. 6-8. 8vo. 1917.
Accountants, Association of—Journal for May 1917. 8vo.
American Chemical Society—Journal for May 1917. 8vo.
Journal of Industrial and Engineering Chemistry for May 1917. 8vo.
American Geographical Society—Geographical Review for May 1917. 8vo.
American Journal of Physiology—Vol. XLIII. Nos. 1-2. 8vo. 1917.
Astronomical Society, Royal—Monthly Notices, Vol. LXXVII. Nos. 5-6. 1917. 8vo.
Bankers, Institute of—Journal, Vol. XXXVIII. Part V. May 1917. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXIV. No. 9. 4to. 1917.
British Astronomical Association—Journal, Vol. XXVII. No. 6. 8vo. 1917.
Buenos Aires—Monthly Bulletin of Municipal Statistics for Nov.-Dec. 1916. 4to.
Canada, Department of Mines—Museum Bulletin, No. 26. 8vo. 1917; Nos. 88 & 94. 8vo. 1916-17.
Geological Survey Memoirs.
Carnegie Institution of Washington—Annual Report of the Director of Department of Terrestrial Magnetism for Year 1916. 8vo.
Chemical Industry, Society of—Journal, Vol. XXXVI. Nos. 8-9. 8vo. 1917.
Chemical Society—Journal for May 1917. 8vo.
East India Association—Journal, New Series, Vol. VIII. No. 2. 8vo. 1917.
Editors—Aeronautical Journal for Jan.-March 1917. 8vo.
Athenæum for May 1917. 4to.
Author for May 1917. 8vo.
Chemical News for May 1917. 4to.
Chemist and Druggist for May 1917. 8vo.
Church Gazette for May 1917. 8vo.
Concrete for May 1917. 8vo.
Dyer and Calico Printer for May 1917. 4to.
Electrical Engineering for May 1917. 4to.
Electrical Industries for May 1917. 4to.
Electrical Times for May 1917. 4to.
Electricity for May 1917. 8vo.
Engineer for May 1917. fol.
Engineering for May 1917. fol.
General Electric Review for May 1917. 8vo.
Horological Journal for May 1917. 8vo.
Illuminating Engineer for May 1917. 8vo.
Journal of Physical Chemistry for April 1917. 8vo.
Journal of the British Dental Association for May 1917. 8vo.
Junior Mechanics for May 1917. 8vo.
Law Journal for May 1917. 8vo.
Marine Magazine for May 1917. 4to.
Model Engineer for May 1917. 8vo.
Musical Times for May 1917. 8vo.
Nature for May 1917. 4to.
New Church Magazine for May 1917. 8vo.
Nuovo Cimento for Sept. 1916. 8vo.

Editors—continued
Page's Weekly for May 1917. 8vo.
Physical Review for April 1917. 8vo.
Power for May 1917. 8vo.
Power-User for May 1917. 8vo.
Science Abstracts for April 1917. 8vo.
Tchèque, La Nation, for April 1917. 8vo.
War and Peace for May 1917. 8vo.
Wireless World for May 1917. 8vo.
Zoophilist for May 1917. 8vo.

Electrical Engineers, Institution of—Journal, Vol. LV. Nos. 265–266. 8vo. 1917.

Florence, Biblioteca Nazionale Centrale—Bollettino for April–May 1917. 8vo.

Franklin Institute—Journal, Vol. CLXXXIII. No. 5, May 1917. 8vo.

Geneva, Société de Physique—Mémoires, Vol. XXXIX. Fasc. 1. 4to. 1916.

Geographical Society, Royal—Journal, Vol. XLIX. No. 5. 8vo. 1917.

Geographical Society, Scottish—Magazine for May 1917. 8vo.

Geological Society of London—Quarterly Journal, Vol. LXXII. Part 1. 8vo. 1917.
Abstracts of Proceedings, Nos. 1006–8. 8vo. 1917.

Jugoslav Committee—Southern Slav Bulletin, No. 30. 1917.

Liverpool, Literary and Philosophical Society of—Proceedings, Vol. LXIV. 8vo. 1916.

London County Council—Gazette for May 1917. 4to.

London Society—Journal for May 1917. 8vo.

Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. LX. Part 3. 8vo. 1917.

Meteorological Office—Weekly Weather Reports for April–May 1917. 4to.
Daily Readings for March 1917. 4to.
Monthly Weather Reports for April 1917. 4to.
Geophysical Journal for July 1915.

Meteorological Society, Royal—Journal, Vol. XLIII. April 1917. 8vo.

Microscopical Society, Royal—Journal, April 1917. 8vo.

Monaco, Musée Oceanographique—Bulletin, Nos. 323–325. 8vo. 1917.

Montpellier Académie des Sciences—Bulletin, Nos. 2–4. 1917. 8vo.

New South Wales, Royal Society of—Journal and Proceedings, Vol. L. Part 1. 8vo. 1916.

New York, Society for Experimental Biology—Proceedings, Vol. XIV. Nos. 5–6. 8vo. 1917.

New Zealand, High Commissioner for—Patent Office Journal, May 1917. 8vo.
Statistics, 1915, Vol. II. 4to.

Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for March–April 1917. 4to. 1917.

Pharmaceutical Society of Great Britain—Journal for May 1917. 8vo.

Photographic Society, Royal—Journal, Vol. LVII. No. 5. 8vo. 1917.

Physical Society of London—Proceedings, Vol. XXIX. Part 3. 8vo. 1917.

Princeton University Observatory—Contributions from, No. 4. 4to. 1916.

Royal Colonial Institute—United Empire, Vol. VIII. No. 5. 8vo. 1917.

Royal Engineers' Institute—Journal, Vol. XXV. No. 6. 8vo. 1917.

Royal Irish Academy—Proceedings, Vol. XXXIII. Section A, No. 6; Section B, Nos. 4–6; Section C, Nos. 12–19. 8vo. 1917.

Royal Society of Arts—Journal for May 1917. 8vo.

Royal Society of London—Proceedings, A, Vol. XCIII. Nos. 649–650; B, Vol. LXXXIX. No. 620. 8vo. 1917.

Selborne Society—Selborne Magazine for May 1917. 8vo.

Smithsonian Institution—Miscellaneous Collections, Vol. LXVI. Nos. 11–13, 15. 8vo. 1916.

Società degli Spettroscopisti Italiani—Memorie, Series 2, Vol. VI. Disp. 2. 4to. 1917.

Statistical Society, Royal—Journal, Vol. LXXX. Part 2, March 1917. 8vo.

Stonyhurst College Observatory—Results of Meteorological, Magnetical and Seismological Observations, 1916. 8vo. 1917.
United Service Institution, Royal—Journal for May 1917. 8vo.
United States Army, Surgeon-General's Office—Index-Catalogue of the Library, Second Series, Vol. XXI. 8vo. 1916.
United States, Department of Agriculture—Journal of Agricultural Research, Vol. IX. Nos. 3–7, 1917. 8vo.
Experiment Station Record, Vol. XXXVI. Nos. 4–5. 8vo. 1917.
United States Department of the Interior—Geological Survey, Bulletins, Nos. 627, 630, 635, 636, 638, 640 B, D, E; 641 B, C, D, E; 645, 649.
Professional Papers: Nos. 91; 98 I, J, K, M, N.
Water Supply Papers: Nos. 360, 384, 387, 395. 8vo. 1916.
Mineral Resources, 1915, Part 1, Nos. 1, 3, 4, 5, 7; Part 2, Nos. 12–14, 16–17, 19–20. 8vo. 1916.
Upsala, Royal Meteorological Observatory—Bulletin, Vol. XLVIII. 4to. 1916–17.
Western Australia, Agent-General—Statistical Abstracts, Aug.–Nov. 1916. 4to.

WEEKLY EVENING MEETING,

Friday, May 11, 1917.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Professor John Joly, M.A. D.Sc. F.R.S.

Radioactive Haloes.

These minute objects have long been known to petrologists, but their explanation is of recent date. They are formed around particles containing radioactive matter and are caused by an effect of the alpha radiations on certain minerals of the ferro-magnesium group. Radioactive haloes may, therefore, be derived from uranium or thorium, and the series of alpha-ray changes characteristic of the radioactive transformations originated by these parent substances determine the dimension and structure of the halo. It is easy to refer a well developed halo to one or other of these parent substances.

The uranium series of elements gives rise to eight alpha-ray emitting substances, each of special and characteristic range. The thorium series of elements gives rise to seven alpha-rays of characteristic range. It is found that when the integral ionization of these rays, as defined by the Bragg curve of ionization for each ray, is determined by the simple process of adding the ordinates of the eight or seven several curves plotted to the same axes of reference, the two resulting curves (i.e. that for the uranium series and that for the thorium series) agree with the features of uranium and thorium haloes.

But in order to account for the mode of development of the halo, i.e. the sequence in which its several features appear, it is further necessary to assume that some cause exists which favours the development of the outer features, or in other words counteracts the weakening of the ionization arising from the spreading of the rays from the radioactive nucleus of the halo. In it is found that the inner ring which marks the first beginning of the halo may be attended by the appearance of the outermost ring due to the alpha-ray of greatest range; an effect which should not occur if the ionization intensity fell off with the divergence of the rays, as might be expected. The explanation offered is that the halo-genesis follows similar laws to those governing photographic effects, so that repetition of stimuli leads to reversal of the earlier effects. Reversal will therefore be less active as the rays diverge outwards, and herein is found a reason for the relative accentuation of the ionization due to the rays of longest range. Haloes presenting all the appearance of more complete reversal have been observed.

The foregoing principles satisfactorily explain the features, dimensions, and order of formation of haloes formed around radioactive centres.

A third type of halo is observed which is referable to the emanation of radium as parent substance. This halo appears generally in connexion with conduits in which there is plain evidence that radioactive gas or liquid has at one time circulated. The nucleus of the halo has apparently absorbed the emanation, and the further changes of this substance, giving rise to four characteristic alpha-rays, have sufficed to form a halo which finds, in its every feature, explanation according to the principles outlined above.

There is some degree of misfit in the primal rings of both the thorium and the uranium halo. In the case of the thorium halo this misfit is very minute, but is sufficient to suggest that the range generally accepted for the thorium alpha-ray is a little excessive. This agrees with the indication of the Geiger-Nutall curve, connecting range and period of transformation. In the case of the primal ring of the uranium halo—which is largely due to the ionization arising from U_1 and U_2—the misfit is more conspicuous, and is in the opposite sense to the misfit of the thorium ray. The observed halo-ring indicates a former range greater than what is now observed.

Until this misfit is shown to be confined to halves of Palæozoic or Archæan ages it seems premature to advance any explanation of it. It is worth noting, however, that a former longer range of the ray of U_1, and hence a more rapid rate of decay of uranium in early times, would explain the disagreement of the lead-ratio of the uranium series with that of the thorium series, and concurrently would reconcile radioactive methods of determining the earth's age with those based on the indications of denudative effects.

[J. J.]

WEEKLY EVENING MEETING,

Friday, May 18, 1917.

SIR WILLIAM PHIPSON BEALE, BART., K.C. M.P.,
Vice-President, in the Chair.

PROFESSOR FREDERICK SODDY, M.A. F.R.S.

The Complexity of the Chemical Elements.

THE elements of the chemist are now known to be complex in three different senses. In the first sense the complexity is one that concerns the general nature of matter, and therefore of all the elements in common to greater or less degree. It follows from the relations between matter and electricity which have developed gradually during the past century as the result of experiments made and theories born within the four walls of this Institution. Associated initially with the names of Davy and Faraday, they have only in these days come to full fruition as the result of the very brilliant elucidation of the real nature of electricity by your distinguished Professor of Physics, Sir Joseph Thomson. Such an advance, developing slowly and fitfully, with long intervals of apparent stagnation, needs to be reviewed from generation to generation, disentangled from the undergrowth that obscures it, and its clear conclusions driven home. This complexity of the chemical elements is a consequence of the condition that neither free electricity nor free matter can be studied alone, except in very special phenomena. Our experimental knowledge of matter in quantity is necessarily confined to the complex of matter and electricity which constitutes the material world. This applies even to the "free" elements of the chemist, which in reality are no more free then than they are in their compounds. The difference is merely that, whereas in the latter the elements are combined with other elements, in the so-called free state they are combined with electricity. I shall touch but briefly on this first aspect, as in principle it is now fairly well understood. But its consistent and detailed application to the study of chemical character is still lacking.

The second sense in which the elements, or some of them at least, are known now to be complex has, in sharp contrast to the first, developed suddenly and startlingly from the recognition in radioactive changes, of different radio-elements, non-separable by chemical means, now called isotopes. The natural corollary of this is that the chemical element represents rather a type of element, the members of the type being only chemically alike. Alike they are in most of those properties, which were studied prior to the last decade of last century and which are due, as we now think, to the outer shells of the atom, so alike that all the criteria, hitherto relied upon by the chemist as being the most infallible and searching, would declare them to be identical. The apparent identity goes even deeper into the region reached by X-ray spectrum analysis which fails to distinguish between them. The difference is found only in that innermost region of all, the nucleus of the atom, of which radioactive phenomena first made us aware.

But, though these phenomena pointed the way, and easily showed to be different what the chemist and spectroscopist would have decided to be identical, it did more. It showed that although the finer and newer criteria, relied upon by the chemist in his analysis of matter, must of necessity fail in these cases, being ultimately electrical in character, yet the difference should be obvious in that most studied and distinctive characteristic of all—the criterion by which Dalton first distinguished the different kinds of atoms—the atomic weight. Those who have devoted themselves to the exact determination of these weights have now confirmed the difference in two separate cases, which, in absence of what perhaps they might regard as "preconceived notions," they were unable to discover for themselves. This is the experimental development to which I wish more especially to direct your attention. It indicates that the chemical analysis of matter is, even within its own province, superficial rather than ultimate, and that there are indefinitely more distinct elements than the ninety-two possible types of element accommodated by the present periodic system.

The third sense in which the elements are known to be complex is that which, in the form of philosophical speculations, has come down to us from the ancients, which inspired the labours of the alchemists of the Middle Ages, and which in the form of Prout's hypothesis has re-appeared in scientific chemistry. It is the sense that denies to Nature the right to be complex, and from the earliest times, faith out-stripping knowledge, has underlain the belief that all the elements must be built up of the same primordial stuff. The facts of radioactive phenomena have shown that all the radio-elements are indeed made up out of lead and helium, and this has definitely removed the question from the region of pure speculation. We know that helium is certainly a material constituent of the elements in the Proutian sense, and it would be harmless, if probably fruitless, to anticipate the day of fuller knowledge by atom building and unbuilding on paper. Apart altogether from this, however, the existence of isotopes, the generalisation concerning the Periodic Law that has arisen from the study of radioactive change on the one hand and the spectra of X-rays on the other, and experiments on the scattering of α-particles by matter, do give us for the first time a definite conception as to what constitutes the difference between one element and another. We can say how gold would result from lead or mercury, even though the control of the processes necessary to effect the change still eludes us. The nuclear atom proposed by Sir Ernest Rutherford, even though, admittedly, it is only a general and incomplete beginning to a complete theory of atomic structure, enormously simplifies the correlation of a large number of diverse facts. This and what survives of the old electronic theory of matter, in so far as it attempted to explain the Periodic Law, will therefore be briefly referred to in conclusion.

THE FREE ELEMENT A COMPOUND OF MATTER AND ELECTRICITY.

Although Davy and Faraday were the contemporaries of Dalton, it must be remembered that it took chemists fifty years to put the atomic theory on a definite and unassailable basis, so that neither of these investigators had the benefit of the very clear view we hold to-day. Davy was the originator of the first electro-chemical theory of chemical combination, and Faraday's dictum, "the forces of chemical affinity and electricity are one and the same," it is safe to say, inspires all the modern attempts to reduce chemical character to a science in the sense of something that can be measured quantitatively, as well as expressed qualitatively. Faraday's work on the laws of electrolysis and the discovery that followed from it, when the atomic theory came to be fully developed, that all monovalent atoms or radicles carry the same charge, that divalent atoms carry twice this charge and so on, can be regarded to-day as a simple extension of the law of multiple proportions from compounds between matter and matter to compounds between matter and electricity. Long before the electric charge had been isolated, or the properties of electricity divorced from matter discovered, the same law of multiple proportions which led, without any possibility of escape, to an atomic theory of matter, led, as Helmholtz pointed out in his well-known Faraday lecture to the Chemical Society in this Theatre in 1881, to an atomic theory of electricity.

The work of Hittorf on the migration of ions, the bold and upsetting conclusion of Arrhenius that in solution many of the compounds hitherto regarded as most stable exist dissociated into ions, the realisation that most of the reactions that take place instantaneously, and are utilised for the identification of elements in chemical analysis, are reactions of ions rather than of the element in question, made very familiar to chemists the enormous difference between the properties of the elements in the charged and in the electrically neutral state.

More slowly appreciated, and not yet perhaps sufficiently emphasized, was the unparalleled intensity of these charges in comparison with anything that electrical science can show, which can be expressed tritely by the statement that the charge on a milligram of hydrogen ions would raise the potential of the world 100,000 volts. Or, if we consider another aspect, and calculate how many free hydrogen ions you could force into a bottle without bursting it, provided, of course, that you could do so without discharging the ions, you would find that, were the bottle of the strongest steel, the breech of gun, for example, it would burst, by reason of the mutual repulsion of the charges, before as much was put in as would, in the form of hydrogen gas, show the spectrum of the element in a vacuum tube.

Then came the fundamental advances in our knowledge of the nature of electricity, its isolation as the electron, or atom of negative electricity, the great extension of the conception of ions to explain the conduction of electricity through gases, the theoretical reasoning, due in part to Heaviside, that the electron must possess inertia inversely proportional to the diameter of the sphere on which it is concentrated by reason of the electro-magnetic principles discovered by Faraday, leading to the all-embracing monism that all mass may be of electro-magnetic origin.

This put the coping-stone to the conclusion that the elements as we apprehend them in ordinary matter are always compounds In the "free" state they are compounds of the element in multiple atomic proportions with the electron. The ions, which are the real chemically uncombined atoms of matter, can no more exist free in quantity than can the electrons.

The compound may be individual as between the atom and the electron, or it may be statistical, affecting the total number merely of the opposite charges, and the element presumably will be an insulator or conductor of electricity accordingly. Analogously, with compounds, the former condition applies to unionised compounds such as are met with in the domain of organic chemistry, or ionised, as in the important classes of inorganic compounds, the acids, bases and salts. Just as the chemist has long regarded the union of hydrogen and chlorine as preceded by the decomposition of the hydrogen and chlorine molecule, so he should now further regard the union itself as a decomposition of the hydrogen atom into the positive ion and the negative electron, and a combination of the latter with the chlorine atom.

One of the barriers to the proper understanding and quantitative development of chemical character from this basis is, perhaps, the conventional idea derived from electrostatics, that opposite electric charges neutralise one another. In atomic electricity or chemistry, though the equality of the opposite charges is a necessary condition for existence, there is no such thing as neutralisation, or the electrically neutral state. Every atom being the seat of distinct opposite charges, intensely localised, the state of electric neutrality can apply only to a remote point outside it, remote in comparison with its own diameter. We are getting back to the conception of Berzelius, with some possibility of understanding it, that the atom of hydrogen, for example, may be strongly electro-positive, and that of chlorine strongly electro-negative, with regard to one another, and yet each may be electrically neutral in the molar sense. Some day it may be possible to map the electric field surrounding each of the ninety-two possible types of atom, over distances comparable with the atomic diameter. Then the study of chemical character would become a science in Kelvin's sense, of something that could be reduced to a number. But the mathematical conceptions and methods of attack used in electrostatics for macroscopic distances are ill-suited for the purposes of chemistry, which will have to develop methods of its own.

We have to face an apparent paradox that the greater the affinity that binds together the material and electrical constituents of the atom, the less is its combining power in the chemical sense. In other words, the chemical affinity is in inverse ratio to the affinity of matter for electrons. The helium atoms offer a very simple and instructive case. Helium is non-valent and in the zero family, possessing absolutely no power of chemical combination that can be detected. Yet we know the atom possesses two electrons, for in radioactive change it is expelled without them as the α-particle. The discharge of electricity through it and positive-ray analysis show that the electrons, or certainly one of them, are detachable by electric agencies, although not by chemical agencies. One would expect helium to act as a diad, forming helides analogous to oxides.

Professor Armstrong for long advocated the view that these inert gases really are endowed with such strong chemical affinities that they are compounds that have never been decomposed. They certainly have such strong affinities for electrons that the atom, the complex of the + ion and electrons, cannot be decomposed chemically. Yet, in this case, where the affinity of the matter for the electron is at a maximum, the chemical combining power is absent.

These gases seem to furnish the nearest standard we have to electric neutrality in the atomic sense. The negative charge of the electrons exactly satisfies the positive charge of the matter, and the atomic complex is chemically, because electrically, neutral. In the case of the electro-positive elements, hydrogen and the alkali-metals, one electron more than satisfies the positive charge on the ion, and so long as the equality of opposite charges is not altered, the electron tries to get away. In the case of the electro-negative elements, such as the halogens, the negative charge, though equal presumably to the positive, is not sufficient to neutralise the atom. Hence these groups show strong mutual affinity, one having more and the other less negative electricity than would make the system atomically neutral like helium. The electron explains well the merely numerical aspect of valency. But chemical combining power itself seems to require the idea that equal and opposite charges in the atomic sense are only exactly equivalent in the case of the inert gases. None of these ideas are now new, but their consistent application to the study of chemical compounds seems curiously to hang fire, as though something were still lacking.

It is so difficult for the chemist consistently to realise that chemical affinity is due to a dissociating as well as to a combining tendency and is a differential effect. There is only one affinity, probably, and it is the same as that between oppositely charged spheres. But, atomic charges being enormous and the distances over which they operate in chemical phenomena being minute, this affinity is colossal, even in comparison with chemical standards. What the chemist recognises as affinity is due to relatively slight differences between the magnitude of the universal tendency of the electron to combine with matter in the case of the different atoms. Over all, is the necessary condition that the opposite charges should be equivalent, but this being satisfied, the individual atoms display the tendencies inherent in their structure, some to lose, others to gain electrons, in order, as we believe from Sir Joseph Thomson's teaching, to accommodate the number of electrons in the outermost ring to some definite number. Chemical affinity needs that some shall lose as well as others gain. Chemical union is always preceded by a dissociation. The tendency to combine, only, is specific to any particular atom, but the energy and driving power of combination is the universal attraction of the + for the − change, of matter for the electron.

THE ELECTRICAL THEORY OF MATTER.

Another barrier that undoubtedly exists to the better appreciation of the modern point of view, even among those most willing to learn, is the confusion that exists between the earlier and the present attempt to explain the relation between matter and electricity. We know negative electricity apart from matter as the electron. We know positive electricity apart from the electron, the hydrogen ion and the radiant helium atom or α-particle of radioactive change for example, and it *is* matter in the free or electrically uncombined condition. Indeed, if you want to find matter free and uncombined, the simple elementary particle of matter in the sense of complexity being discussed, you will go, paradoxically, to what the chemist terms a compound rather than to that which he terms the free element. If this compound is ionised completely it constitutes the nearest approach to matter in the free state. Thus all acids owe their common acidic quality to really free hydrogen, the hydrogen ion, a particle more different from the hydrogen atom than the atom is from the hydrogen molecule.

Positive electricity is thus emphatically not the mere absence of electricity, and any electrical theory of matter purporting to explain matter in terms of electricity does so by the palpable sophistry of calling two fundamentally different things by the same name. The dualism remains whether you speak of matter and electricity, or of positive and negative electricity, and the chemist would do well to stick to his conception of matter, until the physicist has got a new name for positive electricity which will not confuse it with the only kind of electricity that can exist apart from matter.

On the other hand, the theory of the electro-magnetic origin of mass or inertia is a true monism. It tries to explain consistently two things—the inertia of the electron and the inertia of matter—by the same cause. The inertia of the former being accounted for by the well-known electro-magnetic principles of Faraday, by the assumption that the charge on the electron is concentrated into a sphere of appropriate radius; the 2000-fold greater inertia of the hydrogen ion, for example, can be accounted for by shrinking the sphere to one-two-thousandth of the electronic radius.

But the electrical dualism remains completely unexplained. Call the electron E and the hydrogen ion H. The facts are that two E's repel one another with the same force and according to the same law as two H's repel each other, or as an H attracts an E. These very remarkable properties of H and E are not explained by the explanation of the inertia. Are E and H made up of the same stuff or of two different stuffs? We do not know, and certainly have no good reason to assume, that matter minus its electrons is made of the same thing as the electron. We have still to reckon with two different things.

THE CHEMICAL ELEMENTS NOT NECESSARILY HOMOGENEOUS.

I pass now to the second and most novel sense in which the elements, or some of them at least, are complex. In their discovery of new radioactive elements, M. and Mme. Curie used radioactivity as a method of chemical analysis precisely as Bunsen and Kirchoff, and later Sir William Crookes, used spectrum analysis to discover caesium and rubidium, and thallium. The new method yielded at once, from uranium minerals, three new radio-elements, radium, polonium and actinium. According to the theory of Sir Ernest Rutherford and myself, these elements are intermediate members in a long sequence of changes of the parent element uranium In a mineral the various members of the series must co-exist in equilibrium, provided none succeed in escaping from the mineral, in quantities inversely proportional to their respective rates of change, or directly proportional to their periods of average life. Radium changes sufficiently slowly to accumulate in small but ponderable quantity in a uranium mineral, and so it was shown to be a new member of the alkaline-earth family of elements, with atomic weight 226·0, occupying a vacant place in the Periodic Table. Polonium changes 4500 times more rapidly, and can only exist to the extent of a few hundredths of a milligram in a ton of uranium mineral. Actinium also, though its life period is still unknown, and very possibly is quite long, is scarce for another reason, that it is not in the main line of disintegration, but in a branch series which claims only a few per cent of the uranium atoms disintegrating. In spite of this, polonium and actinium have just as much right to be considered new elements, probably, as radium has. Polonium has great resemblances in chemical character both to bismuth and tellurium, but was separated from the first by Mme. Curie and from the second by Marckwald. In the position it occupies as the last member of the sulphur group, bismuth and tellurium are its neighbours in the Periodic Table. Actinium resembles the rare-earth elements, and most closely lanthanum, but an enrichment of the proportion of actinium from lanthanum has been effected by Giesel. The smallness of the quantities alone prevents their complete separation in the form of pure compounds as was done for radium.

The three gaseous members, the emanations of radium, actinium and thorium, were put in their proper place in the Periodic Table almost as soon as radium was, for, being chemically inert gases, their characterisation was simple. They are the last members of the argon family, and the fact that there are three of about the same atomic weight was probably the first indication, although not clearly appreciated, that more than one chemical element could occupy the same place in the Periodic Table.

The extension of the three disintegration series proceeded apace; new members were being continually added, but no other new radio-elements—new, that is, in possessing a new chemical character—were discovered. The four longest-lived to be added, radio-lead or radium-D, as it is now more precisely termed, and ionium in the uranium series, and mesothorium-I and radiothorium in the thorium series, could not be separated from other constituents always present in the minerals, radium-D from lead, ionium and radiothorium from thorium, and mesothorium-I from radium. An appreciable proportion of the radioactivity of a uranium mineral is due to radium-D and its products, and its separation would have been a valuable technical achievement, but, though many attempts have made, this has never been accomplished, and, we know now, probably never will be.

Seven years ago it was the general opinion in the then comparatively undeveloped knowledge of the chemistry of the radio-elements, that there was nothing especially remarkable in this. The chemist is familiar with many pairs or groups of elements, the separation of which is laborious and difficult, and the radio-chemist had not then fully appreciated the power of radioactive analysis in detecting a very slight change in the proportions of two elements, one or both of which were radioactive. The case is not at all like that of the rare-earth group of elements, for example, in which the equivalent or atomic weight is used as a guide to the progress of the separation. Here the total difference in the equivalent of the completely separated elements is only a very small percentage of the equivalent, and the separation must already have proceeded a long way before it can be ascertained.

Human nature plays its part in scientific advances, and the chemist is human like the rest. My own views on the matter developed with some speed when, in 1910, I came across a new case of this phenomenon. Trying to find out the chemical character of mesothorium-I, which had been kept secret for technical reasons, I found it to have precisely the same chemical character as radium, a discovery which was made in the same year by Marckwald, and actually first published by him. I delayed my publication some months to complete a very careful fractional crystallisation of the barium-radium-mesothorium-I chloride separated from thorianite. Although a great number of fractionations were performed, and the radium was enriched, with regard to the barium, several hundred times, the ratio between the radium and mesothorium-I was, within the very small margin of error possible in careful radioactive measurements, not affected by the process. I felt justified in concluding from this case, and its analogy with the several other similar cases then known, that radium and mesothorium-I were non-separable by chemical processes, and had a chemical character not merely like, but identical. It followed that some of the common elements might similarly be mixtures of chemically identical elements. In the cases cited, the non-separable pairs differ in atomic weight from 2 to 4 units. Hence the lack of any regular numerical relationships between the atomic weights would on this view follow naturally. (Trans. Chem. Soc. 1911, xcix. 72.) This idea was elaborated in the Chemical Society's Annual Report on Radioactivity for 1910, in the concluding section summing up the position at that time. This was I think the beginning of the conception of different elements identical chemically, which later came to be termed "isotopes," though it is sometimes attributed to K. Fajans, whose valuable contributions to radioactivity had not at that date commenced, and whose first contribution to this subject did not appear till 1913.

In the six or seven years that have elapsed the view has received complete vindication. Really, three distinct lines of advance converged to a common conclusion, and, so far as is possible, these may be disentangled. First, there has been the exact chemical characterisation from the new point of view of every one of the members of the three disintegration series, with lives over one minute.

Secondly, came the sweeping generalisations in the interpretation of the Periodic Law. Lastly, there has been the first beginnings of our experimental knowledge of atomic structure, which got beyond the electronic constituents and at the material atom itself.

In pursuance of the first, Alexander Fleck, at my request, commenced a careful systematic study of the chemical character of all the radio-elements known of which our knowledge was lacking or imperfect, to see which were and which were not separable from known chemical elements. Seldom can the results of so much long and laborious chemical work be expressed in so few words. Every one, that it was possible to examine, was found to be chemically identical either with some common element or with another of the new radio-elements. Of the more important characterisations, mesothorium-II was found to be non-separable from actinium, radium-A from polonium, the three B-members and radium-D from lead, the three C-members and radium-E from bismuth, actinium-D and thorium-D from thallium. These results naturally took some time to complete, and became known fairly widely to others working in the subject before they were published, through A. S. Russell, an old student, who was then carrying on his investigations in radioactivity in Manchester. Their interpretation constitutes the second line of advance.

Before that is considered, it may first be said that every case of chemical non-separability put forward has stood the test of time, and all the many skilled workers who have pitted their chemical skill against Nature in this quest have merely confirmed it. The evidence at the present day is too numerous and detailed to recount. It comes from sources, such as in the technical extraction of mesothorium from monazite, where one process is repeated a nearly endless number of times; from trials of a very great variety of methods, as, for example, in the investigations on radium-D and lead by Paneth and von Hevesy; it is drawn from totally new methods, as in the beautiful proof by the same authors of the electro-chemical identity of these two isotopes; it is at the basis of the use of radioactive elements as indicators for studying the properties of a common element, isotopic with it, at concentrations too feeble to be otherwise dealt with, and from large numbers of isolated observations, as well as prolonged systematic researches. One of the finest examples of the latter kind of work, the Austrian researches on ionium, will be dealt with later. The most recent, which appeared last month, is by T. W. Richards and N. F. Hall, who subjected lead from Australian carnotite, containing therefore radium-D, to over a thousand fractional crystallisations in the form of chloride, without appreciably altering the atomic weight or the β-activity. So that it may be safely stated that no one who has ever really tested this conclusion now doubts it, and after all they alone have a right to an opinion.

This statement of the non-separability by chemical methods of pairs or groups of elements suffers perhaps from being in a negative form. It looks too much like a mere negative result, a failure, but in reality it is one of the most sweeping positive generalisations that could be made. Ionium we say is non-separable from thorium, but every chemist knows thorium is readily separated from every other known element. Hence, one now knows every detail of the chemistry of the vast majority of these new radio-elements by proxy, even when their life is to be measured in minutes or seconds, as completely as if they were obtainable, like thorium is, by the ton. The difference it makes can only be appreciated by those who have lived through earlier days, when, in some cases dealing with the separation of radio-constituents from complex minerals, after every chemical separation one took the separated parts to the electroscope to find out where the desired constituent was.

As the evidence accumulated that we had to deal here with something new and fundamental, the question naturally arose whether the spectrum of isotopes would be the same. The spectrum is known, like the chemical character, to be an electronic rather than mass phenomenon, and it was to be expected that the identity should extend to the spectrum. The question has been tested very thoroughly by A. S. Russell and R. Rossi in this country, and by the Austrian workers at the Radium Institut of Vienna, for ionium and thorium, and by various workers for the various isotopes of lead. No certain difference has been found, and it may be concluded that the spectra of isotopes are identical. This identity probably extends to the X-ray spectra, Rutherford and Andrada having shown that the spectrum of the γ-rays of radium-B is identical with the X-ray spectrum of its isotope, lead.

THE PERIODIC LAW AND RADIOACTIVE CHANGE.

The second line of advance interprets the Periodic Law. It began in 1911 with the observation that the product of an α-ray change always occupied a place in the Periodic Table two places removed from the parent in the direction of diminishing mass, and that in subsequent changes where α-rays are not expelled the product frequently reverts in chemical character to that of the parent, though its atomic weight is reduced 4 units by the loss of the α-particle, making the passage across the table curiously alternating. Thus the product of radium (Group II) by an α-ray change is the emanation in the zero group, of ionium (Group IV), radium, and so on, while, in the thorium series, thorium (Group IV) produces by an α-ray change mesothorium-I (Group II), which, in subsequent changes in which no α-rays are expelled, yields radio-thorium, back in Group IV again. (Chemistry of the Radio-Elements, p. 29, 1st Edition, 1911.) Nothing at that time could be said about β-ray changes. The products were for the most part very short-lived and imperfectly characterised chemically, and several lacunæ still existed in the series masking the simplicity of the process. But early in 1913 the whole scheme became clear, and was pointed out first by A. S. Russell, in a slightly imperfect form, independently by K. Fajans from electrochemical evidence, and by myself, in full knowledge of Fleck's results, still for the most part unpublished, all within the same month of February. It was found that, making the assumption that uranium-X was in reality two successive products giving β-rays, a prediction Fajans and Göhring proved to be correct within a month, and a slight alteration in the order at the beginning of the uranium series, every α-ray change produced a shift of place as described, and every β-ray change a shift of one place in the opposite direction. Further and most significantly, when the successive members of the three disintegration series were put in the places in the table dictated by these two rules, it was found that all the elements occupying the same place were those which had been found to be non-separable by chemical processes from one another, and from the element already occupying that place, if it was occupied, before the discovery of radioactivity. For this reason the term "isotope" was coined to express an element chemically non-separable from the other, the term signifying "the same place."

So arranged, the three series extended from uranium to thallium, and the ultimate product of each series occupied the place occupied by the element lead. The ultimate products of thorium should, because six α-particles are expelled in the process, have an atomic weight 24 units less than the parent, or about 208. The main ultimate product of uranium, since eight α-particles are expelled in this case, should have the atomic weight 206. The atomic weight of ordinary lead is 207·2, which made it appear very likely that ordinary lead was a mixture of the two isotopes, derived from uranium and thorium. The prediction followed that lead, separated from a thorium mineral, should have an atomic weight about a unit higher, and that separated from uranium minerals about a unit lower, than the atomic weight of common lead, and in each case this has now been satisfactorily established.

THE ATOMIC WEIGHT OF LEAD FROM RADIOACTIVE MINERALS.

It should be said that Boltwood and also Holmes had, from geological evidence, both decided definitely against it being possible that lead was a product of thorium, because thorium minerals contain too little lead, in proportion to the thorium, to accord with their geological ages. Whereas, the conclusion that lead was the ultimate product of the uranium series had been thoroughly established by geological evidence, and has been the means, in the hands of skilful investigators, of ascertaining geological ages with a degree of precision not hitherto possible. Fortunately I was not deterred by the *non possumus*, for it looks as if everybody was right! An explanation of this paradox will later be attempted. In point of fact, there are exceedingly few thorium minerals that do not contain uranium, and since the rate of change of uranium is about 2·6 times that of thorium, one part of uranium is equal as a lead-producer to 2·6 parts of thorium. Thus Ceylon thorianite, one of the richest of thorium minerals, containing 60 to 70 per cent of ThO_2, may contain 10 to 20 and even 30 per cent of U_3O_8, and the lead from it may be expected to consist of very similar quantities of the two isotopes, to be in fact very similar to ordinary lead. I know of only one mineral which is suitable for this test. It was discovered at the same time as thorianite, and from the same locality—Ceylon thorite, a hydrated silicate containing some 57 per cent of thorium and 1 per cent of uranium only. In the original analysis no lead was recorded, but I found it contained 0·4 per cent, which, if it were derived from uranium only, would indicate a very hoary ancestry, comparable, indeed, with the period of average life of uranium itself. On the other hand, if (1) all the lead is of radioactive origin, (2) is stable, and (3) is derived from both constituents, as the generalisation being discussed indicated, this 0·4 per cent of lead should consist 95·5 per cent of the thorium isotope and 4·5 per cent of the uranium isotope. Thorite thus offered an extremely favourable case for examination.

In preliminary experiments in conjunction with H. Hyman, in which only a gram or less of the lead was available, the atomic weight was found relatively to ordinary lead to be perceptibly higher, and the difference, rather less than one-half per cent, was of the expected order.

I was so fortunate as to secure a lot of 30 kilos. of this unique mineral, which was first carefully sorted, piece by piece, from admixed thorianite and doubtful specimens. From the 20 kilos. of first grade thorite, the lead was separated, purified, reduced to metal, and cast *in vacuo* into a cylinder, and its density determined together with that of a cylinder of common lead similarly purified and prepared. Sir Ernest Rutherford's theory of atomic structure, to be dealt with in the latter part of this discourse, and the whole of our knowledge as to what isotopes were, made it appear probable that their atomic volumes, like their chemical character and spectra, should be identical, and therefore that their density should be proportional to their atomic weight. The thorite lead proved to be 0·26 per cent denser than the common lead. Taking the figure 207·2 for the atomic weight of common lead, the calculated atomic weight of the specimen should be 207·74.

The two specimens of lead were fractionally distilled *in vacuo*, and a comparison of the atomic weights of the two middle fractions made by a development of one of Stas's methods. The lead was converted into nitrate in a quartz vessel, and then into chloride by a current of hydrogen chloride, in which it was heated at gradually increasing temperature to constant weight. Only single determinations have been done, and they gave the values 207·20 for ordinary lead, and 207·694 for the thorite lead, figures that are in the ratio of 100 to 100·24. This therefore favoured the conclusion that the atomic volume of isotopes is constant.

At the request of Mr. Lawson, interned in Austria, and continuing his researches at the Radium 'Institut under Prof. Stefan Meyer, the first fraction of the distilled thorite lead was sent him, so that the work could be checked. He reports that Professor Hönigschmid has carried through an atomic weight determination by the silver method, obtaining the value 207·77 ± 0·014, as the mean of eight determinations. Hence, the conclusion that the atomic weight of lead derived from thorite is higher than that of common lead has been put beyond reasonable doubt.

Practically simultaneously with the first announcement of these results for thorium lead, a series of investigations were published on the atomic weight of lead from uranium minerals, by T. W. Richards and collaborators at Harvard, Maurice Curie in Paris, and Hönigschmid and collaborators in Vienna, which show that the atomic weight is lower than that of ordinary lead. The lowest result hitherto obtained is 206·046, by Hönigschmid and Mlle. Horovitz for the lead from the very pure crystallised pitchblende from Morogoro (German East Africa), whilst Richards and Wadsworth obtained 206·085 for a carefully selected specimen of Norwegian clevite. Numerous other results have been obtained, as, for example, 206·405 for lead from Joachimsthal pitchblende, 206·82 for lead from Ceylon thorianite, 207·08 for lead from monazite, the two latter being mixed uranium and thorium minerals. But the essential proportion between the two elements has not, unfortunately, been determined. Richards and Wadsworth have also examined the density of their uranium lead. In every case they have been able to confirm the conclusion that the atomic volume of isotopes is constant, the uranium lead being as much lighter as its atomic weight is smaller than common lead. Many careful investigations of the spectra of these varieties of lead show that the spectrum is absolutely the same so far as can be seen.

THORIUM AND IONIUM.

A second quite independent case of a difference in atomic weight between isotopes has been established. It concerns the isotopes thorium and ionium, and it is connected in an important way with the researches which, on two previous occasions, I have given an account of here, the researches on the growth of radium from uranium, which have been in progress now for fourteen years. It is the intervention of ionium and its very long period of life which has made the experimental proof of the production of radium from uranium such a long piece of work. Previously only negative results were available. One could only say, from the smallness of the expected growth of radium, that the period of average life of ionium must be at least 100,000 years, forty times longer than that of radium, and, therefore, that there must be at least forty times as much ionium by weight as radium in uranium minerals, or at least 13·6 grams per 1000 kilos. of uranium. Since then further measurements, carried out by Miss Hitchins last year, have shown definitely for the first time a clear growth of radium from uranium in the largest preparation, containing 3 kilos. of uranium, and this growth, as theory requires, is proceeding according to the square of the time. In three years it amounted to 2×10^{-11} grams of radium, and in six years to just four times this quantity. From this result it was concluded that the previous estimate of 100,000 years for the period of ionium, though still of the nature of a minimum rather than a maximum, was very near to the actual period.

Joachimsthal pitchblende, the Austrian source of radium, contains only an infinitesimal proportion of thorium. An ionium preparation separated, by Auer von Welsbach, from 30 tons of this mineral, since no thorium was added during the process, was an extremely concentrated ionium preparation. The atomic weight of ionium—calculated by adding to the atomic weight of its product, radium, four for the α-particle expelled in the change—is 230, whereas that of thorium, its isotope, is slightly above 232. The question was whether the ionium-thorium preparation would contain enough ionium to show the difference. Hönigschmid and Mlle. Horovitz have made a special examination of the point, first redetermining as accurately as possible the atomic weight of thorium and then that of the thorium-ionium preparation from pitchblende. They found 232·12 for the atomic weight of thorium, and by the same method 231·51 for that of the ionium-thorium. A very careful and complete examination of the spectra of the two materials showed for both absolutely the same spectrum and a complete absence of impurities.

If the atomic weight of ionium is 230, the ionium-thorium preparation must, from its atomic weight, contain 30 per cent of ionium and 70 per cent of thorium by weight. Professor Meyer has made a comparison of the number of α-particles given per second by this preparation with that given by pure radium, and found it to be in the ratio of 1 to 200. If 30 per cent is ionium, the activity of pure ionium would be one-sixtieth of that of pure radium, its period some sixty times greater, or 150,000 years. This confirms in a very satisfactory manner our direct estimate of 100,000 years as a minimum, and incidentally raises rather an interesting question.

My direct estimate involves directly the period of uranium itself, and if the value accepted for this is too high, that for the ionium will be correspondingly too low. Now, last week, Professor Joly was bringing before you, I believe, some of his exceedingly interesting work on pleochroic halos, from which he has grounds for the conclusion that the accepted period of uranium may be too long. But since I obtained, for the period of ionium, a minimum value two-thirds of that estimated by Meyer from the atomic weight, it is difficult to believe that the accepted period of uranium can have been overestimated by more than 50 per cent of the real period. The matter could be pushed to a further conclusion if it were found possible to estimate the percentage of thorium in the thorium-ionium preparation, a piece of work that ought not to be beyond the resources of radio-chemical analysis. This would then constitute a check on the period of uranium as well as on that of ionium. Such a direct check would be of considerable importance in the determination of geological ages.

The period of ionium enables us to calculate the ratio, between the weights of ionium and uranium in pitchblende, as 17·4 to 10^8, and the doctrine of the non-separability of isotopes leads directly to the ratio, between the thorium and uranium in the mineral, as 41·7 to 10^9. This quantity of thorium is, unfortunately, too small for direct estimation. Otherwise it would be possible to devise a very strict test of the degree of non-separability. As it is, the work is sufficiently convincing. Thirty tons of a mineral containing a majority of the known elements in detectable amount, in the hands of one whose researches in the most difficult field of chemical separation are world-renowned, yield a preparation of the order of one-millionth of the weight of the mineral, which cannot be distinguished from pure thorium in its chemical character. Anyone could tell in the dark that it was not pure thorium, for its α-activity is 30,000 times greater than that of thorium. This is then submitted to that particular series of purifications designed to give the purest possible thorium for an atomic weight determination, and it emerges without any separation of the ionium, but with a spectrum identical with that of a control specimen of thorium similarly purified. The complete absence of impurities in the spectrum show that the chemical work has been very effectively done, and the atomic weight shows that it must contain 30 per cent by weight of the isotope ionium, a result which agrees with its α-activity and the now known period of the latter.

DETERMINATION OF ATOMIC WEIGHTS.

The results enumerated thus prove that the atomic weight can no longer be regarded as a natural constant, or the chemically pure element as a homogeneous type of matter. The latter may be, and doubtless often is, a mixture of isotopes varying in atomic weight over a small number of units, and the former then has no exact physical significance, being a mean value in which the proportions of the mixture as well as the separate atomic weights are both unknown. New ideals emerge and old ones are resuscitated by this development. There may be after all a very simple numerical relation between the true atomic weights. The view that seems most probably true at present is that while hydrogen and helium may be the ultimate constituents of matter in the Proutian sense, and the atomic weights therefore approximate multiples of that of hydrogen, small deviations, such as exist between the atomic weights of these two constituent elements themselves, may be due to the manner in which the atom is constituted, in accordance with the principle of mutual electro-magnetic mass, developed by Silberstein and others. The electro-magnetic mass of two charges in juxtaposition would not be the exact sum of the masses when the charges are separated. The atomic weight of hydrogen is 1·0078 in terms of that of helium as 3·99, and that the latter is not exactly four times the former may be the expression of this effect. Harkins and Wilson have recently gone into the question with some thoroughness, and the conclusion of most interest in the present connection, which appears to emerge, is in favour of regarding most of the effect to occur in the formation of helium from hydrogen, and very little in subsequent aggregations of the helium. In the region of the radio-elements, where we have abundant examples of the expulsion of helium atoms as α-particles, it seems as if we could almost safely neglect this effect altogether. Thus radium has the atomic weight almost exactly 226, and the ultimate product almost exactly 206, showing that in 5 α- and 4 β-ray changes the mean effect is nil, and the atomic weights are moreover integers in terms of oxygen as 16, or helium 4. It is true that the atomic weights of both thorium and uranium are between 0·1 and 0·2 greater than exact integers, but it is difficult to be sure that this difference is real.

When, among the light elements, we come across a clear case of large departure from the integral value, such as magnesium 24·32 and chlorine 35·46, we may reasonably suspect the elements to be a mixture of isotopes. If this is true for chlorine, it suggests a most undesirable feature in the modern practice of determining atomic weights. More and more the one method has come to be relied upon: the preparation of the chloride of the element and the comparison of its weight with that of the silver necessary to combine with the chlorine, and with the weight of the silver chloride formed.

Almost the only practical method, and that a very laborious and imperfect one, which may be expected to resolve a mixture of isotopes, is by long-continued fractional gaseous diffusion, which is likely to be the more effective the lower the atomic weight. Assume, for example, chlorine were a mixture of isotopes of separate atomic weights 34 and 36, or 35 and 36. The 34 isotope would diffuse some 3 per cent faster than the 36, and the 35 some 1·5 per cent faster.

The determination of the atomic weight of chlorine in terms of that of silver has reached now such a pitch of refinement that it should be able to detect a difference in the end fractions of the atomic weight of chlorine, if chlorine or hydrogen chloride were systematically subjected to diffusion. It is extremely desirable that such a test of the homogeneity of this gas should be made in this way.

Clearly a change must come in this class of work. It is not of much use starting with stuff out of a bottle labelled "purissimum" or "garantirt," and in determining to the highest possible degree of accuracy the atomic weight of an element of unknown origin. The great pioneers in the subject, like Berzelius, were masters of the whole domain of inorganic chemistry, and knew the sources of the elements in Nature first-hand. Their successors must revert to their practice and go direct to Nature for their materials, must select them carefully with due regard to what geology teaches as to their age and history, and, before carrying out a single determination, they must analyse their actual raw materials completely, and know exactly what it is they are dealing with. Much of the work on the atomic weight of lead from mixed minerals is useless, for failure to do this. They must rely more on the agreement, or disagreement, of a great variety of results by methods as different and for materials as different as possible, rather than on the result of a single method pushed to the limit of refinement, for an element provisionally purified by a dealer from quite unknown materials. The preconceived notion, that the results must necessarily agree if the work is well done, must be replaced by a system of co-operation between the workers of the world checking each other's results for the same material. A year ago anyone bold enough to publish atomic weight determinations, which were not up to the modern standards of agreement among themselves, would have been regarded as having mistaken his vocation. If these wider ideals are pursued, all the labour that has been lavished in this field, and which now seems to have been so largely wasted, may possibly bear fruit, and where the newer methods fail, far below the narrow belt of elements which it is possible to watch changing, the atomic weight worker may be able to pick up the threads of the great story. No doubt it is writ in full in the natural records preserved by rock and mineral, and the evidence of the atomic weights may be able to carry to a triumphant conclusion the course of elementary evolution, of which as yet only an isolated chapter has been deciphered.

The Structure of the Atom.

The third line of recent advance, which does much to explain the meaning of the isotopes and the Periodic Law, starts from Sir Ernest Rutherford's nuclear theory of the atom, which is an attempt to determine the nature of atomic structure, which again is the necessary preliminary to the understanding of the third aspect in which the elements are or may be complex. That uranium and thorium are built up of different isotopes of lead, helium and electrons is now an experimental fact, since they have been proved to change into these constituents. But the questions how they are built up, and what is the nature of the non-radioactive elements, which do not undergo changes, remain unsolved.

Professor Bragg showed in 1905 that the α-particles can traverse the atoms of matter in their path almost as though they were not there. As far as he could tell, and the statement is still true of the vast majority of α-particles colliding with the atoms of matter, the α-particle ploughs its way straight through, pursuing a practically rectilinear course, losing slightly in kinetic energy at each encounter with an atom, until its velocity is reduced to the point at which it can no longer be detected. From that time, the α-particle became as it were, a messenger that could penetrate the atom, traverse regions which hitherto had been bolted and barred from human curiosity, and on re-emerging could be questioned, as it was questioned, effectively by Rutherford, with regard to what was inside. Sir J. J. Thomson, using the electron as the messenger, had obtained valuable information as to the number of electrons in the atom, but the massive material α-particle alone can disclose the material atom. It was found that, though the vast majority of α-particles re-emerge, from their encounters with the atoms, practically in the same direction as they started, suffering only slight hither and thither scattering due to their collisions with the electrons in the atom, a minute proportion of them suffer very large and abrupt changes of direction. Some are swung round, emerging in the opposite to their original direction. The vast majority, that get through all but undeflected, have met nothing in their passage save electrons, 8000 times lighter than themselves. The few, that are violently swung out of their course, must have been in collision with an exceedingly massive nucleus in the atom, occupying only an insignificant fraction of its total volume. The atomic volume is the total volume swept out by systems of electrons in orbits of revolution round the nucleus, and beyond these rings or shells guarding the nucleus it is ordinarily impossible to penetrate. The nucleus is regarded by Rutherford as carrying a single concentrated positive charge, equal and opposite to that of the sum of the electrons.

Chemical phenomena deal almost certainly with the outermost system of detachable or valency electrons alone, the loss or gain of which conditions chemical combining power. Light spectra originate probably in the same region, though possibly more systems of electrons than the outermost may contribute, while the X-rays and γ-rays seem to take their rise in a deep-seated ring or shell around the nucleus. But mass phenomena, all but an insignificant fraction, originate in the nucleus.

In the original electrical theory of matter, the whole mass of the atom was attributed to electrons, of which there would have been required nearly 2000 times the atomic weight in terms of hydrogen as unity. With the more definite determination of this number, and the realisation that there were only about half as many as the number representing the atomic weight, it was clear that all but an insignificant fraction of the mass of the atom was accounted for. In the nuclear hypothesis this mass is concentrated in the exceedingly minute nucleus. The electro-magnetic theory of inertia accounts for the greater mass if the positive charges that make up the nucleus are very much more concentrated than the negative charges which constitute the separate electrons. The experiments on scattering clearly indicated the existence of such a concentrated central positive charge or nucleus.

The mathematical consideration of the results of α-ray scattering, obtained for a large number of different elements, and for different velocities of α-ray, gave further evidence that the number of electrons, and therefore the + charge on the nucleus, is about half the number representing the atomic weight. But van der Brock, reviving an isolated suggestion from a former paper full of suggestions on the Periodic Law, which were, I think, in every other respect at fault, suggested that closer agreement with the theory would be obtained if the number of electrons in the atom, or the nuclear charge, was the number of the place the element occupied in the Periodic Table. This is now called the atomic number, that of hydrogen being taken as 1, helium 2, lithium 3, and so on to the end of the table, uranium 92, as we now know. For the light elements, it is practically half the atomic weight; for the heavy elements, rather less than half.

I pointed out this accorded well with the law of radioactive change that had been established to hold over the last thirteen places in the Periodic Table. This law might be expressed as follows: The expulsion of the α-particle carrying two positive charges lowers the atomic number by two, while the expulsion of the β-particle, carrying a single negative charge, increases it by one. In ignorance of van der Broek's original suggestion, I had, in representing the generalisation, shown the last thirteen places as differing by unit by unit in the number of electrons in the atom.

Then followed Moseley's all-embracing advance, showing how from the wave-lengths of the X-rays, characteristic of the elements, this conception explained the whole Periodic Table. The square roots of the frequency of the characteristic X-rays are proportional to the atomic numbers. The total number of elements existing betweeen uranium and hydrogen could thus be determined, and it was found to be ninety-two, only five of the places being vacant. The "exceptions" to the Periodic Law, such as argon and potassium, nickel and cobalt, tellurium and iodine, in which an element with higher atomic weight precedes instead of succeeds one with lower, was confirmed by the determination of the atomic numbers in every case. From now on, this number, which represents the + charge on the nucleus, rather than the atomic weight, becomes the natural constant which determines chemical character, light and X-ray spectra, and, in fact, all the properties of matter, except those that depend directly on the nucleus—mass and weight on the one hand, and radioactive properties on the other.

What, then, were the isotopes on this scheme? Obviously they were elements with the same atomic number, the same *nett* charge on the nucleus, but with a differently constituted nucleus. Take the very ordinary sequence in the disintegration series, one α- and two β-rays being successively expelled in any order. Two + and two − charges have been expelled, the *nett* charge of the nucleus remains the same, the chemical character and spectrum the same as that of the first parent, but the mass is reduced 4 units because a helium atom, or rather nucleus, has been expelled as an α-particle. The mass depends on the *gross* number of + charges in the nucleus, chemical properties on the difference between the gross numbers of + and − charges. But the radioactive properties depend not only on the gross number of charges but on the constitution of the nucleus. We can have isotopes with identity of atomic weight, as well as of chemical character, which are different in their stability and mode of breaking up. Hence we can infer that this finer degree of isotopy may also exist among the stable elements, in which case it would be completely beyond our present means to detect. But when transmutation becomes possible such a difference would be at once revealed.

The case is not one entirely of academic interest, because it is probable that the reconciliation of the conflicting views of the geologists and chemists, who concluded that lead was not the ultimate product of thorium, and those who by atomic weight demonstrations on the lead have shown that it is, depends probably on this point.

As has long been known, thorium-C, an isotope of bismuth, disintegrates dually. For 35 per cent of the atoms disintegrating, an α-ray is expelled followed by a β-ray. For the remaining 65 per cent the β-ray is first expelled and is followed by the α-ray. The two products are both isotopes of lead, and both have the same atomic weight, but they are not the same. More energy is expelled in the changes of the 65 per cent fraction than in those of the 35 per cent. Unless they are both completely stable a difference of period of change is to be anticipated.

The same thing is true for radium-C, but here all but a very minute proportion of the atoms disintegrating follow the mode followed by the 65 per cent in the case of thorium-C. The product in this case, radium-D, which, of course, is also an isotope of lead, with atomic weight 210, is *not* permanently stable, though it has a fairly long period, 24 years. The other product is not known to change further, but then, even if it did, it is in such small quantity that it is doubtful whether the change would have been detected. But, so far as is known, it forms a stable isotope of lead of atomic weight 210, formed in the proportion of only 0·03 per cent of the whole.

Now the atomic weight evidence merely shows that *one* of the two isotopes of lead formed from thorium is stable enough to accumulate over geological epochs, and it does not necessarily follow that both are. Dr. Arthur Holmes has pointed out to me that the analysis I gave of the Ceylon thorite leads to a curiously anomalous value for the age of the mineral. The quantity of thorium lead per gram of thorium is 0·0062, and this, divided by the rate at which the lead is being produced, $4 \cdot 72 \times 10^{-11}$ gram of lead per gram of thorium per year, gives the age as 131 million years. But a Ceylon pitchblende, with uranium 72·88 per cent and lead 4·65 per cent, and ratio of lead to uranium as 0·064, gives the age as 512 million years. Dr. Holmes regards the two minerals as likely to be of the same age, and the pitchblende to be, of all the Ceylon results, the one most trustworthy for age measurement.

If we suppose that, as in the case of radium-D, the 65 per cent isotope of lead derived from thorium is *not* stable, and that only the 35 per cent isotope accumulates, the age of the mineral would be 375 million years, which the geologists are likely to consider much more nearly the truth. But the most interesting point is that, if we take the atomic weight of the lead isotope derived from uranium as 206·0, and that derived from thorium as 208·0, and calculate the atomic weight of the lead in Ceylon thorite, assuming it to consist entirely of uranium lead and of only the 35 per cent isotope from thorium, we get the value 207·74, which is exactly what I found from the density, and what Prof. Hönigschmid determined (207·77).

The question remains, if this is what occurs, what does this unstable lead change into? If an α-particle were expelled mercury would result, or if a β-particle bismuth, two elements of which I could find no trace in the lead group separated from the whole 20 kilos. of mineral. But if an α- and a β-particle were both expelled, the product would be thallium, which is present in amount small but sufficient for chemical as well as spectroscopic characterisation. If the process of disintegration does proceed as suggested, it should be possible to trace it, for this particular lead should give a feeble specific α- or β-radiation, in addition, of course, to that due to other lead isotopes. So far it has not been possible to test this. In the meantime, the explanation offered is put forward provisionally as being consistent with all the known evidence.

Looking for a moment in conclusion at the broader aspects of the new ideas of atomic structure, it seems that though a sound basis for further development has been roughed out, almost all the detail remains to be supplied. We have got to know the nucleus, but beyond the fact that it is constituted, in heavy atoms, of nuclei of helium and electrons, nothing is known. Whilst as regards the separate shells or rings of electrons which neutralise its charge and are supposed to surround it, like the shells of an onion, we really know nothing yet at all. The original explanation, in terms of the electron, of the periodicity of properties displayed by the elements, still remains all that has been attempted. We may suppose, as we pass through the successive elements in the table, one more electron is added to the outermost ring for each unit increase in the charge on the nucleus, or atomic number, and that when a certain number, 8 in the early part of the table, 18 later, a complete new stable shell or ring forms, which no longer participates directly in the chemical activities of the atom. Thanks, however, to Moseley's work, this now is not sufficiently precise; for we know the exact number of the elements, and the various atomic numbers at which the remarkable changes, in the nature of the periodicity displayed, occur. Any real knowledge in this field will account not only for the two short initial periods, but also for the curious double periodicity later on, in which the abrupt changes of properties in the neighbourhood of the zero family alternate with the gradual changes in the neighbourhood of the VIIIth groups. The extraordinary exception to the principle of the whole scheme presented by the rare-earth elements remains a complete enigma, none the less impressive because, beyond them again in the table, the normal course is resumed and continues to the end.

[F. S.]

WEEKLY EVENING MEETING,

Friday, May 25, 1917.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.
Treasurer and Vice-President, in the Chair.

J. Barcroft, M.A. F.R.S.

Breathlessness.

[No Abstract.]

WEEKLY EVENING MEETING.

Friday, June 1, 1917.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. F.R.S., President, in the Chair.

J. H. BALFOUR BROWNE, K.C. D.L. J.P. LL.D. M.R.I.

The Brontës: A Hundred Years After.

It is one hundred and one years since Charlotte Brontë was born at Thornton, and ninety-nine years since Emily Brontë was born, and perhaps it is not improper to look upon this year as a sort of centenary of both these remarkable women. There is a "close season" for the dead, and during that period, after the grave has closed upon them, even Calumny shuts its mouth, and to some extent Truth also is muzzled, out of respect to the feelings of the living. It was under this disability—writing in that "close season"—that Mrs. Gaskell achieved her admirable "Life of Charlotte Brontë," which, even with its defects, is perhaps the most readable biography in the language—a biography of which Thackeray said that it "was necessarily incomplete, though most touching and admirable."

It was burdened by the same restrictions that Sir Wemyss Reid compiled, with the assistance of more of the many letters of Charlotte Brontë, his excellent "Monograph," which was published in 1877. Indeed, Sir Wemyss Reid, writing to me after the publication of his book, said he had been accused of "not telling the whole truth about Charlotte's residence in Brussels," and added, "But how could I, whilst her husband still lives, and favours me with an occasional letter of a by no means amicable kind."

But now, after these sixty years, it is easier to speak the whole truth than it was then, and even to point to errors which have been made by writers of "lives" in their painstaking but "necessarily incomplete" records of the Brontës.

There is one curious circumstance to be noted in connexion with the Brontë literature, and that is, that in the case—especially of Charlotte—of writing many books there is no end. Biographies, "Monographs," "Notes," "Circles" and "Mysteries" load the shelves of our libraries, and the curious thing is that all these have been devoted to recording and explaining the life of the most autobiographical of all our novelists. Of course any writer of fiction is, even when pretending to write about the lives of others, making certain confessions as to his own thoughts and feelings, and every writer must transcribe from the pages of his or her experience. It is no surprise to us to learn that the wings of imagination are no use without the feet of experience.

But very few writers have made such literal transcripts from her own past as Charlotte Brontë. Many of her pages read like a diary.

Everyone, from the very first, has recognized, in "Jane Eyre" and "Villette" especially, works of great fiction, but also of intimate biographical confession. Charlotte Brontë, in writing to her life-long friend, Ellen Nussey, says, "Thou hast an honest soul as ever animated human carcase, and a clean one, for it is not ashamed of showing its inmost recesses—only be careful with whom you are frank."

It is true of Charlotte herself that she was not ashamed of showing the inmost recesses of her fiery heart—and it is in that that her merit lies—but she was "frank" with the public, and wrote down not only her own painful history but her most secret feelings in these great confessions of her books.

But although it would seem by thus "making," in the old phrase, "a clean breast of it," she had really left nothing for those who essayed her biography to do, it is this very candour which has apparently been the cause of all this vehement literature which has been devoted to her, and to those who were associated with her.

Beginning with the spite of the "Quarterly" Reviewer, who said of the author of "Jane Eyre" that she was "evidently a woman who had long forfeited the society of her own sex" (it was only a woman that could have written that calumny), the memories of these women —Charlotte, "the fiery-hearted vestal of Haworth," according to Swinburne, and Emily, the strongest writer of fiction of the century —have been tormented by works of criticism.

It was this quality of literal candour in Charlotte Brontë's books which made them not only literature but conundrums. The critics and the public were not only interested in the story, they were delighted in the search for the various persons who had sat in Charlotte's studio for such vivid portraiture. But her works were not only a small portrait gallery—they were a gazetteer.

All the places which are mentioned in them were identified. Lowood was Cowan Bridge School; Oakwell Hall was the Fieldhead in "Shirley"; Villette was Brussels; and so on. Indeed, you cannot turn a page but you are in some place which has been identified, or in the presence of someone who had a real existence and was the model for her sometimes too literal sketch. Mr. Brocklehurst was Mr. Carus Wilson; M. Pelet (in "The Professor"), Rochester; Robert Moore and Paul Emanuel were, according to some, all drawn from M. Héger. Again, Jane Eyre, Caroline Helston and Lucy Snowe were, according to those amateur detectives, Charlotte herself; Madame Beck was Madame Héger; Emily Brontë was Shirley Keeldar; Dr. John was Mr. George Smith, one of her publisher's firm; and St. John Rivers was Henry Nussey. The Mr. Macarthy, with his "steady going clerical faults" mentioned at the end of "Shirley" as the successor of the "rampant boistrous" Mr. Malone, is Mr. A. Nichols, who was afterwards her husband. It was these guesses which delighted the readers who have a keen scent for scandal, and some of them were good guesses—indeed, in some cases the portraiture is so exact that none with eyes could miss the likeness.

Much of the literature about Charlotte Brontë has been occupied with this identification of the bodies of the dead, and it has been quite a surprise to some that Charlotte does not seem to have got others that she knew, like Mrs. Gaskell, Harriet Martineau, Sir D. K. Shuttleworth and Thackeray in her Tussaud Waxworks, and even some of them in the Chamber of Horrors with Mr. Brocklehurst and Madame Beck.*

So far has this process been carried, not without excuse in the case of Charlotte, that some have applied the same inquisition to the pages of "Wuthering Heights"; and one writer, of some ability, has found M. Héger again in Heathcliff, and the ravings of Cathy in the distemper of Charlotte Brontë after her very wise return from Brussels. This dissection—for the vivisection which had to be done in the time of Mrs. Gaskell has ceased, and we are now only in a kind of Morgue—has been carried too far. It is not really literary criticism at all, but a spurious kind of silly curiosity. Every writer has to draw, not for inspiration but for facts, on their experiences, and every writer has, like the spider, to make his web by means of his spinneret out of his own inner being. In this way every book, however fictional, if it is great, is history and biography, and in every work the writer writes himself down on every page. Many have no need, as Dogberry had, of the "Watch," whom he asked to "Write me down an ass!" They can do that for themselves!

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* But not only that the critics found that all the characters in her books can be identified with persons then living, they have in their indefatigable researches denied to her the credit of inventing the plots of her stories. Mrs. Gaskell tells what happened in Charlotte's youth in the neighbourhood of Leeds. "A young lady, who held the situation of a governess, had been wooed and married by a gentleman holding some subordinate position in the commercial firm to which the young lady's employer belonged. A year after her marriage, during which she had given birth to a child, it was discovered that he whom she called her husband had another wife. Report now says that the first wife was deranged, and that he had made this an excuse to himself for his subsequent marriage."

But Professor Jack, of the University of Aberdeen, writing in "The Cambridge History of English Literature," has found almost the whole Jane Eyre story in a tale by J. Sheridan Le Fanu in the "Dublin University Magazine" of 1839. There you have the wife that was no wife, the attempt to murder, and all the rest of it.

Enough has been written about Charlotte and Emily Brontë's youth and the drab surroundings of the dull days of their girlhood. Haworth village, which clambers up the hill to the moors, and the moors themselves are well known to every, even casual, student. The bare facts of their uneventful lives in the staring parsonage, surrounded on three sides by graves, are exceedingly simple. The father, Patrick Brontë, was an ambitious Irishman, born in County Down, where at sixteen he became a schoolmaster when really he could have had very little to teach; but ignorance is a fact that never deters instructors.

But having taught Irish boys for five years, he went to Cambridge, then into the Church; had a curacy in Essex, where he had his first love affair, which, like many that are tried with a "prentice hand," did not prosper. After that he was a curate in Shropshire, at Dewsbury, incumbent of Hartshead in Yorkshire, wrote some poems that were not poetry, became Vicar of Thornton, where he married, and where he afterwards speaks of having been happy, and from this he went to Haworth with his family, an ailing wife, six weird children, and his belongings, in seven carts, in 1820. Here through his eccentric days he remained until he died in 1861. He was not in any way remarkable, although he was a good deal out of the common. His conduct as a clergyman may have been excellent. His principles may have been as pure and as stiff as the starched white neck-cloth which made a rampart about his throat and chin, but he was as bleak in his character as Haworth Moor. His eccentricities leaned to madness side. Finding that Tabby had put out some boots to warm at the fire, for the shoes of the little mites who had been out on the moor in the rain had got wet, he burned them, that the children might not be pampered by what the Yorkshire people call "changing their feet." He cut up into shreds his wife's one piece of finery—a dress which had been given her—and which, although it was too fine to wear, was often looked at with a pleased gleam of satisfaction. He stuffed the hearth-rug up the chimney, and was properly nearly choked by the pungent smoke. He took his meals alone, and after the death of his gentle timid wife, of cancer, he left the children to themselves to build castles in the grim and nipping Yorkshire air, or sent them to starve at the Cowan Bridge School. He carried a pistol about with him for self-defence, but used to discharge it at a harmless barn door when his feelings required something more than ordinary articulate expression. A sort of swearing by deputy. There was nothing very admirable, nothing very remarkable, nothing very lovable about the dour old man. He was like good timber which had been warped in the drying, and as far as his religion went it was more of what the Scotch call "a bee in his bonnet" than the adventure of a soul.

His wife, Miss Branwell of Penzance, was estimable as a weak gentlewoman, who had the heart-ability to discern something to love and respect in her irascible husband, and died when her little children were too young to know a mother's love. Her sister came to Haworth to mother or stepmother his children after Mrs. Brontë's death. She never took kindly to the austere Yorkshire home to which she came, and her aging spinster thoughts seem to have been much with Cornwall. Anne Brontë must have taken after her mother, and never had any of the wild-fire of her father in her. She was a gentle, almost pretty, woman. She, like her more famous sisters, wrote. She wrote two novels which are not very praiseworthy—they were written out of the penury of her experiences. "Agnes Grey" was a small canto in the Odyssey of the Governess, to which Charlotte made more notable contributions; and "The Tenant of Wildfell Hall," which was a novel with a purpose, and was informed with a good deal of her own bitter home experiences of a brother who had been spoiled by his father and his sisters, and was spoiled far more by himself. Her books do not really repay the reading of them, and she died sadly enough with the family disease of "home sickness" as an intercurrent phase of her consumption, strong upon her, at Scarborough, in a shy May of 1849. She was, we may say, a "sweet woman," but we generally say that when there is nothing else to be said. Her poems are pious and prosy, but I believe some of them survive as lugubrious hymns.

Of Branwell Brontë, the son, it is difficult to speak with the patience that his father seems to have suffered him. He had talent, and succeeded in nothing he put his unsteady hand to. He was going to be an artist, but seems to have had none of the gifts which would have carried him to success in that direction. He was an unsatisfactory railway booking clerk, and as private tutor he achieved disgrace. The originality of his death—if it is true that he stood up to face death and died on his feet—is dramatic, but it is too theatrical to impress one with admiration of a youth who was content to be the boon-companion of chance guests at the "Black Bull"—the pot-house in the village—where it is supposed he seasoned poor liquor with better wit. He had a reputation amongst those who found pleasant social intercourse at funeral feasts, or "arvills," but as for the proofs of the genius he was credited with by his sisters, they are not to be found. His letter to Wordsworth, which has been preserved by Mrs. Gaskell, is clever and bumptious, but the verses he sent with it, which have also been preserved, are trash.

That he was a burden in this humdrum parsonage we can well believe, but that his sottishness or opium-eating and disgrace was the cause of Charlotte Brontë's two years of suffering and sorrow after her return from Brussels, as Mrs. Gaskell would have us believe, is for those who look at the evidence far from the true verdict.

Just as their father had been a teacher, there was nothing for it but that all the three girls should be governesses; but they all nourished the ambition to rise to the proud position of keeping a school! Of Emily's life and Anne's there is, so far as their doings are concerned, nothing more to say. They were sent to a school, where they were half-starved. They had to walk two miles—perishing cold miles in winter—to church to hear Mr. Brocklehurst (I beg his pardon, Mr. Carus Wilson) preach. Then there was an epidemic of fever, and no wonder, if it is true, as Sir Wemyss Reid says, that "during the whole time of their sojourn there the young Brontës scarcely ever knew what it was to be free from the pangs of hunger." It was there, too, that the poor little girls had their hair cropped and wore night-caps, but "where pocket handkerchiefs did not appear in the list of clothing."

At home at Haworth they had no children friends, and depended upon one another for all the society they had. To see the little children going out on to the moors hand-in-hand is sad enough, but it is even more melancholy to see them, when on one occasion they had been asked out, or when the village school children came to the parsonage, having to be taught to play. Taught to play! Why they had not learned one of the best lessons life has to teach. Even when Charlotte went to Miss Wooler's school at Roehead she could not play ball with the other girls, but stood about while they played, with the near short-sighted companionship of a book. But they were good little creatures, as quiet as a mouse in the house, and we hear of Maria, when she was about eight, shutting herself up in the study (which had no fireplace) with a newspaper, and reading it all so well that she could even tell you what was in the Parliamentary debates—which is poor food even for a strong stomach, but for a child of eight it is a porridge of sand and straw. There was too much of these Politics and too little of "Nuts and May" in their little lives. When Charlotte became a pupil at Roehead, while they found that she had never learned grammar and very little geography, she knew the names of two ministries, which must have been a puzzle even then, and would be worse in our time of Coalition and National Cabinets. But this isolation of the children accounted for a great deal. A few impressions, like a sharp knife, cut deep. It is the many impressions, which come and go to those more in the world than these little recluses of Haworth, that leave the mind with nothing but scratches on memory. Affections, too, cannot both be wide and deep. If you spread your heart in gold-beater's leaf over many people, the result is acquaintanceship. If you give it to a few it is love! It was thus that the affectionate ties of the Brontë family were as strong as steel. It was thus, too, that Charlotte Brontë, when she did really love someone outside the home circle, loved with an intensity of passion which thrilled her "to the finest fibre of her nature." We may have pity for this grim ordeal of the mites, but we must recognize that this haggard human life was the school which was training the women who were to write such great romances as "Jane Eyre" and "Wuthering Heights."

They were too old for their age, too wise for their years, and began to write and act little plays when they should have been romping. Their games, if they could be called games, were poor imitations of the pastimes of the old and effete, rather than the great games of exuberant childhood. But these hard experiences had the effect of making them shy and reserved with strangers. Charlotte had women friends, but they were rather satellites than equals—Ellen Nussey, Mary Taylor and Margaret Wooler, were life-long worshippers of hers. They shine in her light. She wrote hundreds of letters to Miss Nussey which, although written for private eyes, have been spread out by enterprising biographers before the public. But Charlotte, notwithstanding these companionships, was always a sad and subdued woman—except in the home circle. With strangers she was shrinking and shy, as an untamed squirrel. When she was visiting Mrs. Gaskell at Manchester a Mr. Potter called. Mrs. Gaskell rose to welcome him, and turned round to the chair near the window to present Charlotte Brontë to him. She was surprised to find the chair empty, and she thought Charlotte had escaped through the door into the dining-room. When Mr. Potter left Charlotte appeared from behind the heavy curtain which hung from the window, and explained that she "wasn't able to face a stranger." And yet that was after she was a famous writer of books. But her aloofness was nothing to that of Emily. She doesn't seem to have had any women friends, and no men lovers. She was as lonely as a great Alp! But, after all, is not that one of the dooms of greatness, that it cannot find its mate?

Are not some great spirits severed from friends by the austerity of their genius, and condemned to the solitary cell of their own great imaginings?

Now, it is strange, in relation to Charlotte Brontë, that she should have made one of the fullest confessions of the most secret heart-matters that was ever made by the pen of a women. In her books she not only "stands and unfolds herself," but as we have seen, every place where she has been, most of the persons she has met, are in this her show. That she courted seclusion, and yet should let the public into the closet of her heart, is a very strange fact in literary history. It is, as I said, partly because of the fact that her pictures are veritable bits of domestic history—that the people in them are real people; the actual schools and private houses are the "properties" of her theatre—that she has attained a publicity and secured an interest which is not given to ordinary imaginative literature, and which is withheld from Emily Brontë's more remarkable book. It is because of this characteristic of Charlotte Brontë's books that they have been the centre of a storm of criticism. She has had ardent admirers and angry readers. Her portraiture was often too true to be pleasant. Often she was scathingly unjust, as in her portrait of Madame Beck, if, as we are compelled to believe, that was a likeness of Madame Héger.

Charlotte Brontë's letters have tramped through auction rooms, even a pitiful wisp of hair has been sold at an auction, there are stacks of her correspondence at the British Museum, and there is scarcely a fact in her private timid life that has not been exposed. We are even told, with the trivial minuteness of those who pride themselves on irrelevant details, that she made a chemise when she was five years of age. All this lime-light business would have been very distasteful to a woman with such sensitive modesty as the writer of these masterful books. But she herself has pulled the string of this shower-bath of words and criticisms, and she cannot complain of the douche.

But if, as I have said, their childhood was sad, if they were isolated on those moors, if all their games were lessons, and if they never were the wholesome laughing children they ought to have been, their womanhood was equally depressing and uncomfortable. It requires a saint or a mother to be a good governess. The only one of them to come near saintliness was Anne, and she succeeded better in that vocation than the other two. But motherliness was not in Charlotte Brontë's nature, or, as far as we can make out, in Emily's. The latter liked dogs better than children. It is certain Charlotte never took to children, and there are no real nursery children in her books—nursery children who begin to lose their wings when they begin to feel their feet.*

But this want of motherliness in Charlotte Brontë's books—who was a good house-mother at home—is a distinct defect in these great monuments of genius.

They were great teachers of men and women, but quite poor teachers of little children. It was obvious that neither Charlotte nor Emily made ideal governesses. It is odd to note that with such a dull home as Haworth, the Brontë's were, when they were away from it, home-sick. Thus, Emily, who went as a pupil to Miss Wooler's at Roehead, when Charlotte returned there as a teacher, "became literally ill from home-sickness," and Charlotte says, "I felt in my heart she would die, if she did not go home, and with this conviction obtained her recall." Then, when after some time, Emily went as a governess to a school near Halifax, although there is not in existence any complaint from the young stoic, Charlotte writes of her residence there, "I have had one letter from her since her departure; it gives an appalling account of her duties; hard labour from six in the morning to eleven at night, with only one half-hour of exercise between them. This is slavery: I fear she cannot stand it." She was there only six months. Governessing was a failure. But they still looked forward to the school they were to keep, and it was with a view to preparing themselves for the duties that, with pecuniary assistance from Miss Branwell, they went to school in Brussels in 1842.

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* Swinburne in his "Note," where he does more than eloquent justice to Charlotte Brontë's genius, and in doing so, like an impassioned critic, has done less than justice to other great writers, has acknowledged rightly George Eliot's superior claims in this respect, and admits George Eliot's adorable fidelity in writing of children and even mentions some of her little ones who have the charm of infancy which secures for them unquestioned immortality. That he was sometimes harsh in his critical comparisons is true. In speaking of Charlotte Brontë's work, he says it will exist "when even Daniel Deronda has gone the way of all waxworks; when Miss Broughton no longer 'cometh up as a flower'; and even Mrs. Oliphant is at length 'cut down like the grass.'"

It is there that we are on the threshold of the drama. Before she went to Brussels Charlotte had been asked in marriage—he called it, but it was in housekeeperage—by a stick of a clergyman who was the brother of her friend, Ellen Nussey.

Mrs. Gaskell has it that the proposal was "quietly declined and put aside. Matrimony did not enter into her scheme of life, but good sound earnest labour did." But that seems to me an inept observation. That she quietly put aside a wooden block of a man who wanted a housekeeper more than a wife seems to me the result of her shrewd common sense. But no woman with a burning heart can leave matrimony out of her scheme of life; and as for earnest labour, although she worked well, she wanted more than work, and was athirst for sympathy. I find, too, from a letter from Wemyss Reid, which he wrote to me after the publication of his "Monograph," that "as a schoolgirl, she had seen a certain Yorkshire squireen, whose name has never been mentioned by Mrs. Gaskell or myself, from whom she painted Rochester. This person had unquestionably attracted her fancy as a schoolgirl, but never her love."

But the real experience of her life was to come to her after she had been a year in Brussels. Now she was a woman grown, and in such a one affections can take deeper root than in a schoolgirl. And what a deep root her love for M. Héger, or Paul Emanuel, struck in her heart, can be gathered not only from "Villette," but from the remarkable letters to M. Héger, which only saw the full light—for Mrs. Gaskell only quoted a few words from them—in 1913. Well, what happened in Brussels? She was a pupil, and afterwards a teacher, at £16 a year in Madame Héger's *pensionnat*, and that, as Sir T. Wemyss Reid tells us, was the "turning-point in her life, which changed its current and gave it a new purpose, a new meaning. She learnt much during her two years' sojourn in the Belgian capital, but the greatest of all lessons she mastered while there was that self-knowledge, the taste of which is so bitter to the mouth, though so wholesome to the life."

In a word, she fell in love with M. Héger, and that love, quite unsought by him, unreturned by him, was the deepest experience of her life, and left great scars, such as are the memory of wounds, on the quick heart of this wonderful woman.

Her marriage with Mr. Nicholls need not trouble her biographer. She was only a wife for a few months. She does not seem to have married that gentleman—we can say it now—for love, the love of which her books and letters are full to dazzling; but to secure a help to her eccentric old father, and for herself and her cinder of a heart, a man she could respect. Before her marriage she wrote, "I trust the demands of feeling and duty will be in some manner reconciled by the step in contemplation."

"My destiny," again she writes, "will not be brilliant, certainly, but Mr. Nicholls is conscientious, affectionate, pure in heart and life. He offers a most constant and tried attachment. I am very grateful to him. I mean to try to make him happy, *and papa too.*"

What is all this? The first quotation about "reconciling feeling and duty," and the last, why it reads like the character of a domestic servant who is seeking a place. Still again here is an encomium, "Mr. Nicholls is a kind, considerate fellow, with all his masculine faults; he enters into my wishes about having *the thing* done quietly." The thing is the marriage.

This is very different from the words she, as Lucy Snowe, uses as to Paul Emanuel, "magnificent minded, grand hearted, dear, faulty little man," or again her heart speaks, "Unknown and unloved, I held him harsh and strange—the low stature, the wiry make, the angles, the darkness, the manner, displeased me. Now, penetrated with his influence, and living by his affection, having his worth by intellect, and his goodness by heart—I prefer him before all humanity."

But she could not have said less, poor woman, of Mr. Nicholls. It reads like the description of someone who had paid a morning call, not of the marriage with your heart's desire.

Even four months after her marriage she writes of her husband, "people don't compliment me as they do Arthur—excuse the name, it has grown natural to use it now." Excuse the name! Why this silly diffidence? But even later she writes of him, "He is well, thank God, and so am I, and he is 'my dear boy,' certainly—dearer now than he was six months ago. In three days we shall actually have been married that length of time." This gourd of love is of slow growth.

Mr. Nicholls was doubtless all that Charlotte Brontë—we never think of her as anything else—says of him, but he has really no interest for those who love and believe in the woman he, as if by accident, married. He consoled himself after her death (the last of all the six Brontës) by a marriage with a cousin, and I cannot doubt that it was a more suitable match of mediocrities than his first venture in that reckless direction. Most marriages, if they are made in Heaven, are certainly made in the dark, and a common man who marries a famous woman takes a perilous step. To borrow lustre from her fame, and to have one's private life submitted to the curiosity of a gaping public must be trying for the pachydermatous self-conceit of any man.

But it is not with such experiences as that, or with the apron-string-checked affection of Mr. George Smith (her Dr. John) that we need concern ourselves. As to Dr. John, I may say in passing that it is quite easy to believe that he was as estimable as she made him out to be; but as Wemyss Reid told me, "Charlotte's common sense, assisted by Dr. John's mother, who dreaded the idea of such a union, came to the rescue, and she quietly put her good-looking and prosperous adorer aside." He seems, from her account, to have been a genial companion, but he was a publisher, and the payment of only £500 for the copyright of "Villette," after the success of "Jane Eyre" and "Shirley," was no doubt good business for the firm, but it naturally disappointed the author. But this, of course, was long before the Brussels days, and was, after all, a trivial episode in Charlotte Brontë's life.

Emily seems to have had a heart which could love as deeply, as tragically, as her sister, but seems to have died heart-whole. Still, that for a passionate heart is the worst of diseases.

Charlotte was certainly not heart-whole when she died, although she had got over to some extent the fracture which was the most poignant experience of her womanhood.

There are few such tragedies connected with literature as this one of Charlotte Brontë's. On the background of that forbidding village on the hill-side of the grey Yorkshire moor, after her return from Brussels, there was tragic pain and suffering. Deaths! the history of the Brontës is little more than an obituary! The vicarage at Haworth was there surrounded with hundreds of gravestones, records of hundreds of deaths. But death itself is not a tragedy. It is the futility of life which calls for tears, and there are no annals in all the range of literature of such sorrowful lives as those of the great sisters which were lived in the loneliness and seclusion of that paltry place. Of course it would be absurd to say that lives which produced "Wuthering Heights," "The Old Stoic," "Jane Eyre" and "Villette" were wasted. They were abundantly used. They have left us magnificent legacies. But although the writers have gone home, have they taken their wages? No! Their childhood was wasted, for they never had a child's joy; their womanhood was wasted, for their great heart stores were all in vain; and when they might have accomplished even greater things than these remains give us, they were claimed by death, who had a mortgage of tubercle upon their property in life.

There is one curious matter to be noted in connexion with Charlotte Brontë's life and literature. No writer, as I say, has written more about herself than she has. It is not that she has forced facts into her service—she has made confessions like her own confession to the priest in St. Gudule in Brussels. She has un-packed her heart for her readers. Now after such, sometimes harrowing, candour, it is strange to learn that she was surrounded with mystery. Mrs. Gaskell was naturally reticent about many matters, with the fine taste of an artist and the instinct of a woman. But after her came all sorts of writings. True, most of them were only gleanings in the field she had harvested. From these, however, we hear of the Brussels mystery, and even more recently we have had a book which is called the "Secret of Charlotte Brontë." The now famous Love Letters which were presented to the British Museum by Dr. Paul Héger, the son of the Dr. Héger of Charlotte's days, and which were published in the *Times* at the end of July 1913, were by some thought to clear up doubt and to settle questions as to the life of our novelist which had existed before that date. But even now we find there are some learned quarrels over the poor life that passed away sixty years ago, some asserting that Charlotte Brontë was in love with M. Héger, some that there never was anything more upon her part than admiration for a genius; and, curiously, each of these disputants asserts that his theory is established by the letters she wrote to M. Héger in 1844 and 1845.

Now, the fact is there never was any mystery, and there never was any secret, and those who took the trouble to read Mrs. Gaskell's "Life," Wemyss Reid's "Monograph," and, more important than all, what Charlotte Brontë has herself told us, have not had, could not have had, any doubt about the plain but haggard story of the wonderful little woman. There is nothing that is even unique about her life.

She is not the first woman, and won't be the last, let us hope, who has fallen in love with a man. Not the only woman who has made an ideal out of a quite common-place man, and worships not the real man but her own ideal. That was what happened in Charlotte Brontë's case. Dr. Héger was not the god she thought him. He was quite an ordinary little man with a bad temper, a wife and five children, and considerable ability as a teacher. But he was her ideal, and she breathed into it not only the breath of life but the breath of love, and she "preferred him," her ideal, "before all humanity."

It is quite certain that her love was not asked, was not wanted; indeed, it bored the little Belgian professor when she wrote her passionate hungry letters to him. Of the four which we now possess three of them had been torn up, and in the margin of one of them there was a note giving the address of a Brussel's boot-maker—and yet that letter was a bit of Charlotte Brontë's heart! But, further, even Charlotte's hatred of Madame Héger is not only natural but inevitable. There is no mystery about it. Madame Héger seems to have been a good-looking woman—which Charlotte, with her brow protruding full of thought, was not—the mother of five children, against whom justice cannot find one word to say even after the indictment in the "Professor" and "Villette." Indeed, all recent writers have nothing but praise of the woman. One of them, who was a pupil at the *pensionnat* some twenty years after Charlotte Brontë's time, and who knew them well, has declared that Charlotte, "so far as these two people (M. and Madame Héger) are concerned, adopted an unjust literary and historic method." But for Charlotte she was her hero's wife. She stood very properly between Charlotte, whose "heart was breaking," and M. Héger, who was thinking of a new pair of boots. She may even by a little intellectual shouldering have been trying to push Charlotte Brontë out of the *pensionnat* and back to Haworth, both for Charlotte's own sake and for the sake of her school. All this seems to me as very natural, but in the eyes of a passionate woman like Charlotte Brontë it was enough to make her hate her; and she did! The portrait of Madame Beck in "Villette" is painted from her hate, and not from the life. But so well did Charlotte Brontë hate as well as love, that she was not content to throw ink at Madame Héger in the "Professor," and again in "Villette," we find her even irrelevantly in "Shirley" saying: "I remember seeing a pair of blue eyes that were usually thought sleepy, secretly on the alert, and I knew by their expression—an expression which chilled my blood, it was in that quarter so wonderously unexpected—that for years they had been accustomed to silent soul-reading. The world called the owner of these blue eyes *bonne petite femme* (she was not an English woman). I learned her nature afterwards—got it off by heart, studied it in its furthest and most hidden recesses—she was the finest, deepest, subtlest schemer in Europe." Now this hatred of Madame Héger, which was, as I say, natural enough in this woman, made a poor dictator of many pages in her novels, and these have really led to an unjust view of the character of this woman, that Charlotte not only hated but maligned.

Long afterwards she induced even a sane critic like Wemyss Reid to believe her, and to form quite an erroneous opinion of the woman. In a letter he sent me after the publication of his "Monograph," he said, "Now, do you not see what a painful time that must have been at Brussels? Charlotte, *perfectly* pure in mind and heart, and yet captive to a clever, shrewd, eccentric man, the Paul Emanuel of the picture; dogged day and night by a jealous, lying, unscrupulous old Belgian woman, forced to feed upon herself, for she had no confidante until after her return to England ('I cannot write what I want to say,' she cries in one of her letters); and debarred from every social pleasure, went through a perfect agony of suffering during these dark years. She returned to Haworth a changed and disillusioned woman." Now, I do not think one of the epithets which he applies to Madame Héger is justified. He has been over-persuaded by Charlotte Brontë, who had been quite unfair both in her estimate of the woman, and quite unwise in writing her hate in a book in parallel columns, as it were, to her great unreturned love. Still, I see nothing at all strange in Charlotte Brontë's distortion of the character of the woman she thought her rival—a rival who had rights to which she had no claim. That she was unjust is quite true, but which one of us is quite judicial in all our estimates of other people, especially when these people stand in our way, with or without a flaming sword, barring our entrance into Eden?

I see no mystery in all this. There is no secret to unfold. There is no one to be scolded or condemned. M. Héger was blind, and Charlotte did not understand his blindness and callousness when she was begging the crumbs of words which might fall from his table. But there is associated with all strong emotions an idea that others are in the same whirlwind as that which is carrying us along. It is a surprise to passion that the outer world seems tranquil, when it is in fierce turmoil. Nothing is a more common delusion in one who loves than that it must be the means of inspiring affection in its object. It was thus with Charlotte Brontë. It was a surprise to her that, while she was in a hot hell of passion, the object of her passion could remain an iceberg.

Madame Héger naturally thought more of her school for young girls than of the English governess, and was wise to prevent, if she could, a breath of scandal which would have dimmed the brassplate on the door of the *pensionnat*; and Charlotte Brontë is not to be blamed for a forward love—for it is love that is her merit—a deep, passionate love that made her a notable woman, and burned till the heat of her genius lifted the lid of silence and steamed over in those three great books.

It is the idea of a prude that love should not be given when it is not asked, and when it is not returned. Love is a free gift, and not a barter. It would be a sorry world if it was only to be sold in the market. Some of the very best affections have been pure charities, some of the noblest loves have been unrequited. But these, although unrequited in the world of sense, are never without their reward. And although Charlotte Brontë thought, on leaving Brussels, that her heart would break, although she beat her poor heart against the bars of silence—as we read in those letters now in the British Museum—and suffered for two years the pain of the damned, it was not for nothing. It was this bitter education which was to make, not mar, the woman who was to write "Jane Eyre" and "Villette."

It is, of course, the height of folly of a critic who wants readers, or a lecturer who wants a listening audience, to declare that there is no secret to disclose, and that the whole story is quite natural, and could have been understood by anyone who took the trouble to listen to Charlotte Brontë herself, not as the writer of an inspired gospel, but as the writer of magnificently prejudiced confessions, which were just or unjust, true or untrue, as the woman herself.

Miss Martineau wrote of her books, "all the female characters in all their thoughts and lives are represented as being full of one thing

—love," and from her own intellectual point of view, she added there are "substantial heart-felt interests for women of all ages in ordinary circumstances, quite apart from love."

That is quite true, but quite irrelevant. It shows only that she did not understand Charlotte Brontë. In answer to it Charlotte wrote: "I know what love is, as I understand it, and if man or woman should be ashamed of feeling such love, then there is nothing right, noble, faithful, truthful, unselfish, in this earth, as I comprehend rectitude, nobleness, fidelity, truth and disinterestedness."

It is this knowing "what love is" that has made her books great, and has left her with an unsullied reputation—notwithstanding her martyrdom for love's sake—for rectitude, nobleness, fidelity, truth and disinterestedness, which has enhanced the world's opinion of womanhood.

Now although, as I have said, we ought from her veritable books to have been in no doubt as to the turning-point in Charlotte Brontë's life, there is one mystery to be cleared up, and that is, how a competent critic and careful student like Mr. Clement Shorter, whose "Brontë" books are as a whole valuable contributions to literary history, should have been betrayed into such a mistake as to Charlotte Brontë's real spiritual relations to M. Héger. In his books he had erroneously, I think, and in the teeth of the evidence and Sir Wemyss Reid's summing up of it, taken up this position—that Charlotte Brontë had admired M. Héger as a master, and respected him as a man of genius, and that such an attitude was quite natural in a woman situated as she was. Even then it struck one that the writer did not know the difference between the passion of love—as Charlotte "knew it"—and the literary toady's lukewarm respect. The temperature of these two is very different when tested even by a not very sensitive thermometer.

It was love that was written large over her books, and not book love, but heart love. What must one make of Charlotte's own statement that "she returned to Brussels against her conscience, prompted by an irresistible impulse, and so lost peace and happiness for two years"? Is this the respect one feels for a good teacher, or the sorrow one suffers from missing his lectures from the *estrade*? But again, what was the meaning of her confession in St. Gudule? What had the poor, spotless governess to confess except her love? It is of this confession she wrote to Emily as "a real confession." "Better not tell papa," she wrote, "he will not understand it was only a freak, and will, perhaps, think I am going to turn Catholic." Not understand it? No one could, unless he had the key to her heart. It was not a freak, it was a "real confession." But not content with the ear of the priest, she has confessed herself with more truth than the sanction of an oath ever gives in "Villette." "Lucy Snowe waited, and hoped and waited, but Paul Emanuel never came back." Her father wanted her to make a happy ending to the book, but how could she, remembering her "loss of peace and happiness for two years"? Had she altered it, it would have changed her veracious work into a trivial lie. One would have thought that these evidences of themselves would have convinced anyone that this was love, and not respect. But then, on the back of all this, came the letters published in 1913, and written during those two years when she had "no peace or happiness." Are these to be explained on this cold hypothesis? "To write to an old pupil cannot be for you an interesting occupation," she wrote to M. Héger, "*but for me it is life.*"

"Life"—is that the way a woman of twenty-eight would address an admired teacher like Huxley or Tyndall?

But again, "And when the sweet and dear consolation of seeing your handwriting, of reading your counsels, fades from me like a vision, then fever attacks me, appetite and sleep fail, I feel that life wastes away."

Further, "Oh, Monsieur, I know I once wrote you a letter that was not a reasonable one, because my heart was clouded with grief, but I will not do it again. I will try not to be selfish, although I cannot but feel your letters are the greatest happiness I know."

Again, "How can I endure my life if I am forbidden to make any effort to alleviate my sufferings?" Is this the dutiful pupil who cannot endure life unless she has her "exercises" corrected by Herr Professor?

"Monsieur, the poor do not need much to keep them alive; they ask only for the crumbs that fall from the rich man's table, but if these crumbs are refused then they die of hunger!" But the whole four of the letters—which were sent, you remember, surreptitiously by hand, and not by post, for Charlotte still believed in the jealousy of Madame Héger, and naturally did not wish such letters to fall into jealousy's hands—were throbbing with affection, and ought to have convinced anyone that this heart-cry was no mere craving for the notice and companionship of a great man, an exalted literary enthusiasm, but was a woman's wailing affection. That these letters could be written in the course of ordinary acquaintanceship is absurd. They are a very sufficient record of lost love, of unrequited affection.

But Mr. Clement Shorter, even after reading these indubitable love-letters, continued to regard Charlotte Brontë as nothing more than a faithful pupil—a hero-worshipper of the professor of literature, instead of a passionate disciple of the man! After reading them, he confided to an interviewer that "they" (the letters) "were actuated only by the immense enthusiasm of a woman desiring comradeship and sympathy with a man of character. There was no sort of great sorrow on her part because Professor Héger was a married man, and it is plain in her letters that she only desired comradeship with a great man." "There is nothing in these letters, published now for the first time, that any enthusiastic woman might not write to a man double her age who was a married man with a family, and who had been her teacher."

Mr. Shorter seems to have a somewhat unusual notion of ordinary correspondence, and I question very much whether it is the ordinary lot of teachers to receive such letters from the enthusiastic admirers of their prelection methods. But Mr. Shorter is wrong in his facts as well as his inferences. Professor Héger was not "double her age," but only eight years older than she was. Charlotte was not what used to be called a "gushing girl," but a woman of twenty-eight. Professor Héger was not a great man, although he was apparently a competent teacher. As for the sorrow that he was a married man, no one has suggested that; but that a pure, honest unmarried woman may hopelessly love a man who is married does not seem to be beyond the bounds of possibility. And that a woman would, under such untoward circumstances, suffer "loss of peace and happiness" we can all believe.

But it is curious that each of the best-known writers about Charlotte Brontë has made a mistake. Mrs. Gaskell, with a tenderness which is perhaps to her credit, thought that those two years of misery—for which Charlotte herself vouches—were due to the anxiety she felt at the unsteady career and sottish downfall of her brother Branwell, and not to the pure passionate heart-hunger of the woman. This explanation of the gloom of these two bad years was rejected by Sir Wemyss Reid and by more recent writers, and there is next to no evidence in support of Mrs. Gaskell's kind but erroneous view of the cause of these storm-clouds. But Sir Wemyss Reid himself, though he corrected Mrs. Gaskell, fell into the error, persuaded to it by "Lucy Snowe," that Madame Héger was a jealous, spying, cruel woman. This, again, as I have said, and as Mrs. Macdonald shows in her short but adequate book, was a mistake; and it is a mistake which did injustice to the memory of a quite worthy woman. That the author of "Villette" made her as black as a pen could make her was natural enough; and there was a further excuse for the author—that Madame Héger was under the thin veil of fictional treatment. But the critic had no right to throw more mud at the dead woman. Mr. Clement Shorter, too, in his mistaken admiration for Charlotte Brontë, tries to make her a blue-stocking saint, instead of a real living woman with a heart which has gone out, like Noah's dove, and found no resting-place on the desolate world of waters.

How these mistakes were possible, in the presence of Charlotte Brontë's own candid and honest confessions in "Villette," and how one of them can be persisted in since the publication of the letters of 1844–1845, which are only, as it were, a chapter in that novel, it is difficult to conjecture. These are, as I have said, brimful of love. It was love that made the pages of "Villette" so veracious, and make much of it stand out now as if it had been written in letters of fire. It was *her* love that was precious. That we are loved is a luxury.

That we love is a necessary education, even if, as in her case, it may be associated with a cruel penance. We see the result of the great ordeal in her books, which, like dropping torches, set other hearts on fire, and can thrill even old hearts which have almost forgotten how to vibrate.

It is well to note that it is passion that is the inspiration of the two great books upon which her fame principally rests. Intellectually Charlotte Brontë was narrow. Her sympathies were for the most part the children of habit, and her religion a strait-waistcoat. After her Brussels' experience, however, her heart spoke out. She was no longer narrowed by her early prejudices or iron habits, or confined by the swaddling bands of her religion. She spoke loudly and deeply to other people who have hearts. It was thus, then, that she was great; it was thus that Harriet Martineau's criticism was as beside the mark as it would be to condemn a rose for having red petals.

Here was her great success. A compound of great passion and deep grief, and that in spite of the many minor faults that critics could find with her works. Many of her expressions are hopelessly clumsy. Her real great books were crude in thought, lacking in humour, and had quite a narrow range of sympathy. A great many women writers of fiction have been her superiors both in art and in intellect; but none of their works, which pass us without touching any deep chord, are comparable with these works of hers, which contain the grandeur of one over-mastering emotion.

It was this great quality which, when "Jane Eyre" first appeared, caused a good deal of unfavourable criticism from the bloodless prudes and the paltry critics; but these had to stand aside—the book might not be praised, but it was read. It is this quality which keeps her words alive to-day.

A favourite question of the *quidnuncs* of literature when a writer dies is, "Will he live?" It is a question which it is impossible to answer. The answer to it depends not on our estimate of the dead man's or woman's literary merit, but on our knowledge of the tastes and intelligence of the future readers of books. It is difficult to judge even of contemporary merits, or to foretell from one day to another the success or failure of a book or a work of art. Thus, we know that six publishers at least refused to have anything to do with Charlotte Brontë's "Professor." We know that the early venture of the lives of Currer, Ellis, and Acton Bell, the publication of their "Poems," which cost them £50, was a misadventure, for, notwithstanding the great excellence of some of these, only two copies of the book were sold! We know that Charlotte Brontë's publishers only paid her in the aggregate £1500 for the entire copyright of her three great novels. Are these facts the measure of greatness? We know, of course, that the publisher sits in the coach, and the author is between the shafts. But when we remember that an author for a novel trivial in comparison with any of these received £10,000, we must abandon money as a test of merit. How, then, if we cannot tell the tastes of to-day, or the fashions of admiration of our own time, are we to foretell the taste or fashions of our successors, when we are asked of Charlotte and Emily Brontë's works, "Will they live?"

All I can say is that if these books die, it is not their fault, for they are vital and ought to live; but it will be your fault, and your children's fault. If they are neglected, it will be because the generations of the future have acquired the taste of a decadent for a literature which is itself in decay.

Far less has been said and written about Emily Brontë than about the author of "Jane Eyre." There are many reasons for this harsh neglect. Emily had more aloofness in her than Charlotte. She was of all the oddities at Haworth Parsonage perhaps the oddest. She had, says one accurate critic, "the eyes of a half-tamed creature," but she had also the *ways* of a half-tamed creature.

I have said little of her life because there is so little to say. She, too, was one of the victims of Cowans Bridge School. She, too, became a governess—a poor lot for a woman tamed like Anne, but intolerable for a soaring eaglet like Emily. Charlotte tells us that at the school near Halifax Emily was in "slavery." But think what slavery must be to a woman who could write—

Riches I hold in light esteem,
  And love I laugh to scorn,
And lust of fame is but a dream,
  That vanished with the morn.

And if I pray, the only prayer
  That moves my lips for me,
Is "Leave the heart that now I bear,
  And give me liberty"!

Yes, as my swift days near their goal,
  'Tis all that I implore,
In life and death a chainless soul,
  With courage to endure.

She was a pupil, too, at the *pensionnat* at Brussels, and one more ingenious than wise writer has, in the search for origins, suggested that not only was M. Héger the Rochester and Paul Emanuel of Charlotte's books, but that he was also the Heathcliff of that withering literature "Wuthering Heights." We have seen that some have told us it was a "great secret" that Charlotte was in love with M. Héger, although she had told us so herself; but under the dire necessity of accumulating mystery where there is none, each new writer has tried to find a new secret with which to whet the blunting curiosity of the public. So we find that Mrs. Chadwick, in her book "In the Footsteps of the Brontës," hints that Emily too was in love with the little, black, fiery and kind Professor of the Brussels Athénée, and tells us that he admired her more than he did Charlotte, on the ground apparently that he said of Emily, "She ought to have been a man, a great navigator"—evidence of affection which, in my opinion, is about as cogent as the famous "Chops and Tomato Sauce," which was, I believe, admitted as evidence in the leading case of "Bardwell v. Pickwick."

All these futile surmises come from the foolish analysis of books for their sources. The origin of a work of genius like "Wuthering Heights" is not the places and the persons who may have been seen and known by the author, but the inspiration is in the writer. It is not the environment which makes the artist, it is the artist who makes the environment.

Charlotte Brontë said of her sister Emily that "she had no model." The haggard players upon her stage are the children of her weird imagination. But here, again, a very competent critic—Sydney Dobell, who described "Wuthering Heights" as "the unformed writing of a giant hand, the large utterance of a baby god"—did make a mistake, and thought that the "giant hand" which wrote both "Jane Eyre" and "Wuthering Heights" was that of Charlotte Brontë. We know that was not the fact. It was a stronger hand than that which wrote "Villette" that wrote that eerie book of passionate tragedy. It was the same hand or "baby god" who wrote "The Old Stoic," "The Last Lines," and "Death!"—a stronger hand even than that which wrote the "Dream of Pilate's Wife."

We know something of Emily from Charlotte's photographic art, for we find her in the brightest, the healthiest, but the most commonplace of her novels, "Shirley." There we have Shirley Keeldar cauterising her own arm when she had been bitten by a dog she believed to be mad. But that is one of the actual facts in Emily Brontë's tragic life. She was a stoic, and bore and suffered the dullness and monotony, which is worse than pain, with a stubborn endurance of a "chainless soul."

To the end she endured. She refused to see a doctor, and was just willing to die. She was not the one to flinch when death came to her. When she could scarcely stand she got up out of bed and tried to comb her hair—it was beautiful hair, and Emily had been the beauty of the family—but so weak was she that the comb fell from her hand into the fire. "See, Martha," she said, "my comb has fallen into the fire and I cannot get it." After that she was so exhausted that she said, "I will see a doctor now." But it was too late to call in a doctor. She leaned on the couch and passed away. She was buried with the others in the Haworth church.

Her fierce and faithful dog, "Keeper," when they came back from the funeral, "lay down at Emily's door and howled pitifully for many days."

The dog had more sense than the British public, which did not know the great loss it had sustained. The writer of "Wuthering

Heights," a book which has some of the force of the play-writers of the great age of Greece in its terrible pages—although slowly recognized in her greatness, although tardily accepted by a certain intelligent public—was, in my opinion, a greater genius than her sister, and has made such a deep mark upon the fictional literature of England that it cannot be erased or obliterated.

That her intellectual stature was higher, that her genius was greater, can be seen not only when they stand side by side in their novels, but when you compare their poetry. Much that Charlotte wrote was good, but none of it was great. Much that Emily wrote is above praise, and goes straight to our acceptance and belief as the work of the highest genius.

Let me, although it is a mistake to patch a lecture with a piece of such supreme merit, for "it taketh away from the garment and the rent is made worse," quote a verse or two :

> No coward soul is mine,
> No trembler in the world's storm-troubled sphere.
> I see Heaven's glories shine,
> And faith shines equal, arming me from fear.
>
> Vain are the thousand creeds
> That move men's hearts; unutterably vain,
> Worthless as withered weeds,
> Or idlest froth amid the boundless main.
>
> With wide-embracing love
> Thy spirit animates eternal years,
> Pervades and broods above,
> Changes, sustains, dissolves, creates and rears.
>
> Though earth and man were gone,
> And suns and universes ceased to be,
> And Thou wert left alone,
> Every existence would exist in Thee.

These are gems of "purest ray serene," from that cavern of the Haworth parsonage. And these, it may be said, like one swallow, do not make a summer ; but they are certainly the heralds of the coming clement season. A girl who could write like this before she was twenty-six years of age, and with only the starved experience of life to her intellectual credit, could have written in time even better; but what one feels about the crudest of her writings is that they were decisive work, not tentative effort; we feel that, with all their defects, and some of these are glaring enough, there was an obvious strength which did not leave you free to agree or disagree, to acquiesce or withhold your critical assent, but which commandeered your admiration and made a slave of your attention. But even her best poetry, which does not bulk large, is not so supreme as the rugged prose in which she has spoken of "Heathcliff and Cathy" in the shuddering pages of "Wuthering Heights." No weirder work of genius ever came from a woman's pen.

Some foolish people have called "Wuthering Heights" a dreadful book, and I have some reason to believe that even now it is not read as it ought to be. Of course, as a story—to say the least of it—it is not prepossessing. In some of its aspects it is almost repulsive. The lurid thunderclouds which hang over the windy moors (that Emily loved) are seldom relieved even by a short lucid interval of sunshine. The hero, Heathcliff, although of course he does not deserve to be called a hero, although human, is possessed of the worst qualities we ascribe to the Devil. But the book blazes with passion—passion sometimes on the verge of insanity. I deny that Emily Brontë had a diseased or a morbid mind, as some have with a near approach to imbecility, asserted. We all have our bad dreams, and this "Wuthering Heights" is one of hers. But it is a dream which does not go when sleep is dissolved in waking, it lives on with us even when day itself dawns to lighten our misery.

Remember that the outer world was very little to Emily Brontë. She held it at arm's length. She banished it. Liberty—the liberty of aloofness, of a recluse--was, as Charlotte said, "the breath of her nostrils." So far as she knew the world, and her experiences were scarcely a handful, she had not found it a companionable place. It was not a bland world. It had scowled at her like an enemy. Even Brussels, which had made a lasting and harrowing impression on Charlotte, seems to have failed to influence Emily in any way whatever. It was the moors and the rough gnarled folk, which, like the stunted prickly thorns, inhabit the hollows in them, that were her most intimate experiences.

Mrs. Dean and Joseph the Calvinist, they are truer to life than any affidavit is to the fact. Joseph, "the wearisomest self-righteous Pharisee that ever ransacked the Bible to rake all the promises to himself and fling the curses to his neighbours," you might find in or near Haworth to-day. But Heathcliff, the two Catherines, and Hareton Earnshaw, although as real, are not folk that have ever been born, but they are people who will never die.

In these respects "Wuthering Heights" is one of the most unique books that has been given to the world—and given by an ignorant, nervous, untrained, untamed girl, who was educated only by Divine inspiration.

It is easy to depict wooden saints. Much even of good literature is full of these in its niches; but to paint a flesh-and-blood man, whose training has stunted any good that was in him, and whose passion has developed into a mania, that is the work of a genius, and it has been accomplished with the ease of strength by Emily Brontë, and by none other. There is no haunted man in all our books so real as Heathcliffe. Of course, "Wuthering Heights" is in prose, rugged prose, but in the result it is a great tragic poem from beginning to end.

These great works of Emily are a cry from a deep passionate heart, but it passes the lips as melancholy music.

There is reason why we Members of the Royal Institution should consider the works of these remarkable women. Sir Humphrey Davy, lecturing here in 1810, after speaking of the absolute necessity of the strenuous cultivation of experimental philosophy in the National Interest, concluded his lecture with a remarkable appeal for the help that women could give to the movement: "Our doors are open to all who wish to profit by knowledge, and I may venture to hope, that even the female parts of our audiences will not diminish, and that they will honour the place with an attention which is independent of fashion, or the taste of the moment, and connected with the use, the permanence and the pleasure of intellectual acquisition."

Here, as we know, women are exactly on the same level as men, and have had our suffrage all these years.

Speaking of women, he said: "Let them make it disgraceful for men to be ignorant, and ignorance will vanish, and that part of their empire founded upon mental improvement will be strengthened and exalted by time, will be untouched by age, and will be immortal in its youth."

He added: "The maxim of improvers is—promote whatever can tend to assist the progress of the human mind, and *Letters will always be the most powerful engine to this effect.* That which is beautiful, that which is pathetic, that which is sublime, can never lose its effect."

It is, encouraged by his eloquent words uttered here more than a hundred years ago, that I have ventured to recall these great, almost the greatest, of women writers to your attention.

[J. H. B. B.]

GENERAL MONTHLY MEETING,

Monday, June 4, 1917.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G., President, in the Chair.

Mrs. E. Harriman Dickinson,
Alfred Dobrée,
Mrs. Florence Stacpoole,

were elected Members.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Report of Board of Scientific Advice for India, 1915-16. 8vo. 1917.
Madras: S. Indian Images of Gods and Godesses. By H. K. Gastri. 8vo. 1916.
Agricultural Journal, Vol. XII. Part 2. 8vo. 1917.
Agricultural Research Institute, Pusa: Bulletin, No. 68. 8vo. 1917.
Astronomer Royal—Report to the Board of Visitors of the Royal Observatory. 8vo. 1916.
Accademia dei Lincei, Roma—Atti, Serie Quinta, Rendiconti: Classe de Scienze Fisiche, Mathematiche e Naturali, Vol. XXVI. 1^o Semestre, Fasc. 9-10. 8vo. 1917.
Accountants, Association of—Journal for April 1917. 8vo.
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American Chemical Society—Journal for June 1917. 8vo.
Journal of Industrial and Engineering Chemistry for June 1917. 8vo.
American Geographical Society—Geographical Review for June 1917. 8vo.
American Journal of Physiology—Vol. XLIII. No. 3. 8vo. 1917.
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British Astronomical Association—Journal, Vol. XXVII. No. 7. 8vo. 1917.
Canada, Royal Society of—Transactions, Third Series, Vol. X. March 1917. 8vo.
Chemical Industry, Society of—Journal, Vol. XXXVI. No. 10. 8vo. 1917.
Chemical Society—Journal for June 1917.
Editors—Athenæum for June 1917. 4to.
Author for June 1917. 8vo.
Chemical News for June 1917. 4to.
Chemist and Druggist for June 1917. 8vo.
Church Gazette for June 1917. 8vo.
Concrete for June 1917. 8vo.
Dyer and Calico Printer for June 1917. 4to.
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Electrical Industries for June 1917. 4to.
Electrical Times for June 1917. 4to.
Electricity for June 1917. 8vo.
Engineer for June 1917. fol.
Engineering for June 1917. fol.
General Electric Review for June 1917. 8vo.
Horological Journal for June 1917. 8vo.
Il Nuovo Cimento for Oct.-Nov. 1916. 8vo.
Illuminating Engineer for April 1917. 8vo.
Journal of Physical Chemistry for May 1917. 8vo.
Journal of the British Dental Association for June 1917. 8vo.
Junior Mechanics for June 1917. 8vo.
Law Journal for June 1917. 8vo.
Model Engineer for June 1917. 8vo.
Musical Times for June 1917. 8vo.
Nature for June 1917. 4to.
New Church Magazine for June 1917. 8vo.
Page's Weekly for June 1917. 8vo.
Physical Review for May 1917. 8vo.
Power for June 1917. 8vo.
Power User for June 1917. 8vo.
Science Abstracts for May 1917. 8vo.
Tcheque, La Nation, for May 1917. 8vo.

Editors—continued
War and Peace for June 1917. 8vo.
Wireless World for June 1917. 8vo.
Zoophilist for June 1917. 8vo.
Florence Biblioteca Nazionale Centrale—Bollettino for June 1917. 8vo.
Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. XIV. Disp. 2. 8vo. 1917.
Franklin Institute—Journal, Vol. CLXXXIII. No. 6. 8vo. 1917.
Glasgow, Royal Philosophical Society—Proceedings, Vol. XLVII. 1915-16. 8vo.
Geographical Society, Royal—Journal, Vol. XLIX. No. 6. 8vo. 1917.
Geological Society of London—Abstracts of Proceedings, No. 1009. 8vo. 1917.
Hipkins, Miss Edith—Collection of Books and Pamphlets on "Musical Pitch," etc.
Kinnear, Miss M.—Lessons on Chemistry. Balmain. 8vo. 1844.
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Weekly Weather Reports for June 1917. 4to.
Daily Readings for April 1917. 4to.
Geophysical Journal for August 1916. 4to.
New Zealand, High Commissioner for—Patent Office Journal, June 1917. 8vo.
Numismatic Chronicle and Journal, 1916—Part 4. 8vo. 1916.
Pharmaceutical Society of Great Britain—Journal for June 1917. 8vo.
Quekett Microscopical Club Journal—Series 2, Vol. XIII. No. 80, 1917. 8vo.
Rome, Ministry of Public Works—Giornale del Genio Civile for Feb.-March 1917. 8vo.
Royal Colonial Institute—United Empire, Vol. VIII. No. 6. 8vo. 1917.
Royal Society of Arts—Journal for June 1917. 8vo.
Royal Society of Edinburgh—Proceedings, Vol. XXXVII. Part 2. 8vo. 1917.
Transactions, Vol. LI. Parts 2-3. 4to. 1915-17.
Royal Society of London—Philosophical Transactions, Series B, Vol. CCVIII. No. 353; Series A, Vol. CCXVII. No. 552.
Sanitary Institute, Royal—Journal, Vol. XXXVIII. No. 2. 8vo. 1917.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXIII. No. 6. 8vo. 1917.
Selborne Society—Selborne Magazine for June 1917. 8vo.
Tôhoku Mathematical Journal—Vol. XI. No. 3. 1917. 8vo.
United States Department of Agriculture—Journal of Agricultural Research, Vol. IX. Nos. 8-10. 1917. 8vo.
Experiment Station Record, Vol. XXXVI. Nos. 6-7. 8vo. 1917.
United States Patent Office—Official Gazette, May 1917. 8vo.
Washington, National Academy of Sciences—Proceedings, Vol. III. No. 5. 8vo. 1917.

GENERAL MONTHLY MEETING,

Monday, July 2, 1917.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G.,
President, in the Chair.

R. D. Pullar,
F. E. Robotham,

were elected Members.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Accademia dei Lincei, Reale, Roma—Rendiconti, Classe di Scienze Fisiche, Mathematiche e Naturali. Serie Quinta, Vol. XXVI. 1° Semestre, Fasc. 11-12; Classe di Scienze Morali. Serie Quinta, Vol. XXV. Fasc. 7-10. 8vo. 1917.
Accountants, Association of—Journal for July 1917. 8vo.
Agricultural Society, Royal—Journal, Vol. LXXVII. 8vo. 1916.
American Chemical Society—Journal for July 1917. 8vo.
Journal of Industrial and Engineering Chemistry for July 1917. 8vo.
American Journal of Philology—Vol. XXXVIII. No. 2. 8vo. 1917.
American Journal of Physiology--Vol. XLIII. No. 4. 8vo. 1917.
American Museum of Natural History—Bibliography of Published Writings of Henry Fairfield Osborn for the Years 1877-1915. 8vo. 1916.
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Astronomical Society, Royal—Monthly Notices, Vol. LXXVII. No. 7. 1917 8vo.
Memoirs, Vol. LXII. Part 1. 4to. 1917.
Boston Public Library -Bulletin, Third Series, Vol. X. No. 11. 8vo. 1917.
Cambridge Philosophical Society--Transactions, Vol. XXII. Nos. 10-11. 4to. 1917.
Canada, Department of Mines—Museum Bulletin, No. 25. 8vo. 1917.
Geological Survey Memoirs, Nos. 93, 97. 8vo. 1917.
Mines Branch, Bulletin, Nos. 14, 17. 8vo. 1917.
Report on the Building and Ornamental Stones of Canada, Vol. IV. 8vo. 1916.
Annual Report on Mineral Production of Canada for the Year 1915. 8vo. 1917.
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East India Association—Journal, New Series, Vol. VIII. No. 3. 8vo. 1917.
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Electrical Industries for July 1917. 4to.
Electrical Times for July 1917. 4to.
Electricity for July 1917. 8vo.
Engineer for July 1917. fol.
Engineering for July 1917. fol.
General Electric Review for July 1917. 8vo.
Horological Journal for July 1917. 8vo.
Illuminating Engineer for July 1917. 8vo.
Journal of the British Dental Association for July 1917. 8vo.
Junior Mechanics for July 1917. 8vo.
Law Journal for July 1917. 8vo.
Model Engineer for July 1917. 8vo.
Musical Times for July 1917. 8vo.
Nature for July 1917. 4to.
New Church Magazine for July 1917. 8vo.
Nuovo Cimento for Dec. 1916–Jan. 1917. 8vo.
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Power for July 1917. 8vo.
Power-User for July 1917. 8vo.
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Terrestrial Magnetism for June 1917. 8vo.
War and Peace for July 1917. 8vo.
Wireless World for July 1917. 8vo.
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Faraday Society—Transactions, Vol. XII. Parts 1–3. 8vo. 1917.
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Geological Society of London—Abstracts of Proceedings, No. 1010. 8vo. 1917.
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Kyōto Imperial University—Memoirs: College of Engineering, Vol. I. Nos. 8–10. 8vo. 1917.
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Daily Readings for May 1917. 4to.
Monthly Weather Reports for June 1917. 4to.
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Microscopical Society, Royal—Journal for June 1917. 8vo.
Milan, R. Scuola Superiore Agricoltura—Annuario, Vol. XIII. 8vo. 1917.
National Academy of Sciences, Washington—Proceedings, Vol. III. No. 6. 8vo. 1917.
New York, Society for Experimental Biology—Proceedings, Vol. XIV. No. 7. 8vo. 1917.
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Statistics, 1915, Vol. III. 4to.
Paris, Académie des Sciences—Comptes Rendus, Tome CLXV. 4to. 1917.
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Philadelphia — Proceedings, Academy of Natural Sciences, Vol. LXVIII. Part 3. 8vo. 1917.
Photographic Society, Royal—Journal, Vol. LVII. No. 6. 8vo. 1917.
Post Office Electrical Engineers, Institution of—Journal, Vol. X. Part 2. 8vo. 1917.
Royal Asiatic Society—Journal, July 1917. 8vo.
Royal Society of Arts—Journal for July 1917. 8vo.
Royal Society of Edinburgh—Proceedings, Vol. XXXVII. Part 3. 8vo. 1917.
Transactions, Series A, Vol. CCXVII. Nos. 553-554. 4to. 1917.
Royal Society of London—Proceedings, A, Vol. XCIII. Nos. 651-652; XCIV. No. 1; B, Vol. LXXXIX. No. 621; C, Vol. XXXIV. Nos. 1-2. 8vo. 1917.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXIII. No. 7. 8vo. 1917.
Selborne Society—Selborne Magazine for July 1917. 8vo.
Smithsonian Institution — Miscellaneous Collections, Vol. LXVI. Nos. 14, 16-18; Vol. LXVIII. No. 4. 8vo. 1917.
Contributions to Knowledge, Vol. XXXV. No. 3. 4to. 1916.
South African Association for Advancement of Science—Report, 1915. 8vo. 1916.
Statistical Society, Royal—Journal, Vol. LXXX. Part 3, May 1917. 8vo.
Tohoku Imperial University, Sendai, Japan—Science Reports, 1st Series, Vol. VI. No. 1. 8vo. 1917.
United States Department of Agriculture—Journal of Agricultural Research, Vol. IX. Nos. 11-12, 1917. 8vo.
Experiment Station Record, Vol. XXXVI. No. 8. 8vo. 1917.
United States, Naval Observatory—Supplement to the American Ephemeris, 1918. 8vo. 1917.
Upsala, Royal Meteorological Observatory—Observations Seismographiques. 8vo. 1917.
Western Australia, Agent-General—Report of the Department of Mines, 1915. 4to. 1917.
Western Society of Engineers—Journal, Vol. XXI. Nos. 9-10; Vol. XXII. No. 1. 8vo. 1916-17.
Wisconsin Academy—Transactions, Vol. XVIII. Part 2. 8vo. 1916.

GENERAL MONTHLY MEETING,

Monday, November 5, 1917.

SIR JAMES CRICHTON-BROWNE, M.D. F.R.S., Treasurer and Vice-President, in the Chair.

The Treasurer announced that the Managers had received an Anonymous Gift of £500 from a Lady Member; and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted:—

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to express to the Lady who has anonymously and unconditionally placed at their disposal, for the purposes of the Institution, the sum of £500, their most grateful appreciation of her munificence and discernment. They accept the Gift as a timely and noble recognition of the good public work the Institution has done in the past, and is still doing, in the acquisition and diffusion of Scientific Knowledge, and as an incitement to maintain and extend its usefulness in the unique position which it has for more than a century occupied.

The Special Thanks of the Members were returned to Richard Pearce, Esq., F.G.S., for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperature.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Report of the Indian Association for the Cultivation of Science for the Year 1915. 8vo. 1917.
Proceedings of Indian Association for the Cultivation of Science, Vol. III. Part 1. 8vo. 1917.
Geological Survey of India: Records, Vol. XLVII. Part 4. 8vo. 1916; Memoirs, Vol. XLV. Part 1. 8vo. 1917.
Memoirs of the Department of Agriculture: Entomological Series, Vol. V. Nos. 2-3; Botanical Series, Vol. IX. No. 3. 8vo. 1917.
Accademia dei Lincei, Roma—Atti, Serie Quinta: Rendiconti. Classe de Scienze Fisiche, Mathematiche e Naturali, Vol. XXVI 2° Semestre, Fasc. 1-6; Classe di Scienze Morali, Vol. XXV. Fasc. 11-12; Vol. XXVI. Fasc. 1-2. 8vo. 1917.
Accountants, Association of—Journal for Aug.-Sept. 1917. 8vo.
American Chemical Society—Journal for July-Oct. 1917. 8vo.
Journal of Industrial and Engineering Chemistry for Aug.-Oct. 1917. 8vo.
American Geographical Society—Geographical Review for July-Oct. 1917. 8vo.
American Journal of Philology—Vol. XXXVIII. No. 3. 8vo. 1917.
American Journal of Physiology—Vol. XLIII No. 4; Vol. XLIV. Nos. 1-3. 8vo. 1917.
American Philosophical Society—Proceedings, Vol. LVI. Nos. 1-2. 8vo. 1917.
Asiatic Society, Royal—Journal for Oct. 1917. 8vo.
Bombay Branch Journal, Vol. XXIV, No. 3. 8vo. 1917.
Astronomical Society, Royal—Monthly Notices, Vol. LXXVII. Nos. 8-9. 8vo. 1917.
Australia, Census of the Commonwealth of—Vol. I. Appendix A. 4to. 1917.
Bankers, Institute of—Journal, Vol. XXXVIII. Part 7. 8vo. 1917.
Beck, James M., LL.D.—The War and Humanity. 8vo. 1917.
Boston, Society of Natural History—Occasional Papers, No. 13. 8vo. 1915.
Memoirs, Vol. VIII. No. 2. 4to. 1916.
Proceedings, Vol. XXXV. Nos. 2-3. 8vo. 1915.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXIV. Nos. 12-14. 4to. 1917.
British Association for the Advancement of Science—Report of the Eighty-sixth Meeting (Newcastle-on-Tyne, 1916). 8vo. 1917.
British Astronomical Association—Journal, Vol. XXVII. Nos. 8-9. 8vo. 1917.
Memoirs, Vol. XXI. Part 1. 8vo. 1917.
List of Members, 1917. 8vo.
Buenos Ayres—Bulletin of Municipal Statistics for Jan.-June 1917. 4to.
Cambridge Observatory—Report of the Observatory Syndicate, 1915-16. 4to. 1917.
Cambridge Philosophical Society—Proceedings, Vol. XIX. Parts 2-3. 8vo 1917.

Canada, Department of Mines—Preliminary Report of the Mineral Production of Canada during the Calendar Year 1916. 8vo. 1917.
Summary Report of Geological Survey for the Year 1916. 8vo. 1917.
Geological Survey: Memoirs, Nos. 84, 87, 98. 8vo. 1916–17.
Mines Branch: Bulletin, No. 15. 8vo. 1917.
Carnegie Endowment for International Peace—Year-Book for 1917. 8vo.
Chemical Industry, Society of—Journal, Vol. XXXVI. Nos. 14–19. 8vo. 1917.
Chemical Society—Journal for Aug.-Oct. 1917.
Chemistry, Institute of—Proceedings, Part 3, 1917. 8vo.
Chicago. Field Museum of Natural History—Publications: Zoological Series, Vol. X. No. 15, Vol. XII. No. 1; Report Series, Vol. V. No. 2; Anthropological Series, Vol. VI. No. 4. 8vo. 1917.
Chicago, John Crerar Library—Twenty-second Annual Report, 1916. 8vo. 1917.
Consolo, Enrico—The War in Italy, Vols. VII.-IX. 4to. 1917.
Dax, Société de Borda—Bulletin, 1916, Nos. 3–4. 8vo.
Donald, James—New Testament Christianity. By Lancelot Oliver. 8vo. 1911.
Editors—Aeronautical Journal for April-Sept. 1917. 8vo.
Athenæum for Aug.-Oct. 1917. 4to.
Author for Oct. 1917. 8vo.
Chemical News for Aug.-Oct. 1917. 4to.
Chemist and Druggist for Aug.-Oct. 1917. 8vo.
Church Gazette for Aug.-Oct. 1917. 8vo.
Concrete for Aug.-Oct. 1917. 8vo.
Dyer and Calico Printer for Aug.-Oct. 1917. 4to.
Electric Vehicle for Sept. 1917. 8vo.
Electrical Engineering for Aug.-Oct. 1917. 4to.
Electrical Industries for Aug.-Oct. 1917. 4to.
Electrical Times for Aug.-Oct. 1917. 4to.
Electricity for Aug.-Oct. 1917. 8vo.
Engineer for Aug.-Oct. 1917. fol.
Engineering for Aug.-Oct. 1917. fol.
General Electric Review for Aug. 1917. 8vo.
Horological Journal for Aug.-Sept. 1917. 8vo.
Il Nuovo Cimento for Feb.-June 1917. 8vo.
Illuminating Engineer for June-Aug. 1917. 8vo.
Journal of Physical Chemistry for June-Oct. 1917. 8vo.
Journal of the British Dental Association for Aug.-Oct. 1917. 8vo.
Junior Mechanics for Aug.-Oct. 1917. 8vo.
Law Journal for Aug.-Oct. 1917. 8vo.
Model Engineer for Aug.-Oct. 1917. 8vo.
Musical Times for Aug.-Oct. 1917. 8vo.
Nature for Aug.-Oct. 1917. 4to.
New Church Magazine for Aug.-Oct. 1917. 8vo.
Page's Weekly for Aug.-Oct. 1917. 8vo.
Physical Review for July-Sept. 1917. 8vo.
Post Office Electrical Engineers' Journal, Vol. X. Part 3. 8vo. 1917.
Power for Aug.-Oct. 1917. 8vo.
Power User for Aug.-Oct. 1917. 8vo.
Science Abstracts for Aug.-Oct. 1917. 8vo.
Tchèque, La Nation, for July-Sept. 1917. 8vo.
Terrestrial Magnetism for Sept. 1917. 8vo.
War and Peace for Aug.-Oct. 1917. 8vo.
Wireless World for Aug.-Oct. 1917. 8vo.
Zoophilist for Aug.-Oct. 1917. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LV. No. 268. 4to. 1917.
Faraday House—Journal, Vol. VII. No. 4. 8vo. 1917.
Florence Biblioteca Nazionale Centrale—Bollettino for Aug.-Sept. 1917. 8vo.

Formosa, Government of—Icones Plantarum Formosanarum, Vol. VI. 8vo. 1916.
Franklin Institute—Journal, Vol. CLXXXIV. Nos. 1–4. 8vo. 1917.
Geneva, Société de Physique—Comptes Rendus, No. 33, 1916. 8vo 1917.
Mémoires, Vol. XXXVIII. Fasc. 6. 4to. 1917.
Geographical Society, Royal—Journal, Vol. L. Nos. 2-5. 8vo. 1917.
Geological Society of London—Quarterly Journal, Vol. LXXII. Part 2. 8vo. 1917.
List of Members, 1917.
Geological Survey of Great Britain—Summary of Progress, 1916. 8vo. 1917.
Harvard College Observatory—Seventy-first Annual Report of the Director, 1916. 8vo. 1917.
Horticultural Society, Royal—Journal, Vol. XLII. Parts 2–3. 8vo. 1917.
Humanitarian League, The—The Flogging Craze. By Henry S. Salt. 8vo. 1916.
Imperial College of Science—Calendar, 1917-18. 8vo.
Imperial Institute—Bulletin, Jan.-Mar. 1917. 8vo.
Iron and Steel Institute—Journal, Vol. XCV. 8vo. 1917.
Life-Boat Institution, Royal National—Journal for Aug. 1917. 8vo.
Linnean Society—Transactions: Botany, Vol. IX. Part 1; Zoology, Vol. XVII. Part 3. 4to. 1916-17.
Literature, Royal Society of—Transactions, Vol. XXXV. 8vo. 1917.
London County Council—Gazette for Aug.-Oct. 1917. 4to.
London Society—Journal, No. 14. 8vo. 1917.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. LXI. Part 1. 8vo. 1917.
Mechanical Engineers, Institution of—Proceedings, 1917, Jan.-May. 8vo. 1917.
Meteorological Office—Monthly Weather Reports for July-Sept. 1917. 4to.
Weekly Weather Reports for Aug.-Oct. 1917. 4to.
Daily Readings for June-Aug. 1917. 4to.
Geophysical Journal for Dec. 1915; Jan.-March, 1916. 4to.
Meteorological Society, Royal—Journal, Vol. XLIII. No. 183, July. 8vo. 1917.
Metropolitan Asylums Board—Annual Report, 1916. 8vo. 1917.
Microscopical Society, Royal—Journal, 1917, Parts 4-5. 8vo.
Monaco, Musée Océanographique—Bulletin, Nos. 326-328. 8vo. 1917.
Montpellier, Académie des Sciences—Bulletin, Nos. 5-7. 1917. 8vo.
National Physical Laboratory—Collected Research, Vol. XIII. 1916. 4to.
Report for year 1916-17. 4to. 1917.
New South Wales, Royal Society of—Journal and Proceedings, Vol. L. Parts 2-3. 8vo. 1916.
New York Academy of Sciences—Annals, Vol. XXV. pp. 1-308. 8vo. 1916; Vol. XXVII. pp. 193-203, 205-214. 8vo. 1917.
New York, Society for Experimental Biology and Medicine—Proceedings, Vol. XIV. No. 8. 8vo. 1917.
New Zealand, High Commissioner for—Patent Office Journal, July 1917. 8vo.
Statistics, 1915. Vol. IV. 4to. 1916.
Numismatic Chronicle and Journal, 1917, Parts 1-2. 1917. 8vo.
Paris, Academie des Sciences—Tomes 160–162. 4to. 1915–16.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for July-Aug. 1917. 4to.
Pharmaceutical Society of Great Britain—Journal for Aug.-Oct. 1917. 8vo.
Philadelphia, Academy of Natural Sciences—Proceedings, Vol. LXIX. Part 1. 8vo. 1917.
Physical Society of London—Proceedings, Vol. XXIX. Part 5. 8vo. 1917.
Ralli, Mrs. Stephen, M.R.I.—The Political Life of M. Tricoupi, 1898-1916. 8vo. 1917.
Rockefeller Institute for Medical Research—Studies, Vol. XXVI. 8vo. 1917.
Rome, Ministry of Public Works—Giornale del Genio Civile for April-July 1917. 8vo.

Röntgen Society—Journal, Vol. XIII. Nos. 52–53. 8vo. 1917.
Royal College of Surgeons—Calendar, 1917. 8vo.
Royal Colonial Institute—United Empire, Vol. VIII. Nos. 8-10. 8vo. 1917.
Royal Dublin Society—Scientific Proceedings, Vol. XV. Nos. 15–23. 8vo. 1917; Economic Proceedings, Vol. II. Nos. 12-13. 8vo. 1917.
Royal Engineers' Institute—Journal, Vol. XXVI. Nos. 2-5. 1917. 8vo.
Royal Society of Arts—Journal for Aug.-Oct. 1917. 8vo.
Royal Society of Edinburgh—Transactions, Vol. LI. Part 4. 4to. 1915–17.
Royal Society of London—Philosophical Transactions, Series B, Vol. CCVIII. No. 354; Series A, Vol. CCXVII. Nos. 555-558.
Proceedings: Series A, Vol. XCIII, Nos. 653-655; Series B, Vol. LXXXIX, No. 622; Vol. XC, No. 623.
Ryd, V. H.—On Computation of Meteorological Observations. 8vo. 1917.
Sanitary Institute, Royal—Journal, Vol. XXXVIII. No. 3. 8vo. 1914.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXIII. Nos. 8–10. 8vo. 1917.
Selborne Society—Selborne Magazine for Aug.-Sept. 1917. 8vo.
Smithsonian Institution—Miscellaneous Collections, Vol. LXVIII. No. 3. 8vo. 1917.
Società degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. VI. Disp. 5–8. 4to. 1917.
South Africa, Union of—Department of Agriculture, Nos. 1, 3, 4, 7, 9, 12, 18. 8vo. 1916.
South Australian School of Mines and Industries—Annual Report for 1916, and Prospects for 1917. 8vo. 1917.
Southern Slav Bulletin—Nos. 32, 34. 1917.
Statistical Society, Royal—Journal, Vol. LXXX. Part 4. 8vo. 1917.
Tohoku Imperial University, Sendai, Japan—Science Reports, First Series, Vol. VI. No. 2. 8vo. 1917.
Tôhoku Mathematical Journal—Vol. XI. No. 4, and Vol. XII. Nos. 1-2. 8vo. 1917.
Toronto, University of—Biological Series, No. 17; Papers from Physical Laboratories, Nos. 50–57; Papers from Chemical Laboratories, Nos. 108–110; Physiological Series, No. 13. 8vo. 1916–17.
United Service Institution, Royal—Journal for August, 1917. 8vo.
United States Department of Agriculture—Journal of Agricultural Research, Vol. X, No. 1-12. 1917. 8vo.
Experiment Station Record, Vol. XXXVI. No. 9; Vol. XXXVII. Nos. 1–3. 8vo. 1917.
United States Patent Office—Official Gazette, July-Aug. 1917. 8vo.
Vernon, The Hon. W. Warren (the Author)—Lectures on Dantë and his Times. 8vo. 1917.
Recollections of Seventy-two Years. 8vo. 1917.
Washington, National Academy of Sciences—Proceedings, Vol. III. Nos. 7-9. 8vo. 1917.
Western Society of Engineers—Journal, Vol. XXII. Nos. 2–3. 8vo. 1917.
Yorkshire Archæological Society—Journal, Vol. XXIV. Part 95. 8vo. 1917.
Yorkshire Philosophical Society—Annual Report, 1916. 8vo. 1917.
Zoological Society of London—Proceedings, Parts 1-2. 1917. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 3, 1917.

SIR JAMES CRICHTON-BROWNE, M.D. F.R.S., Treasurer and Vice-President, in the Chair.

Frederick Chamberlin,
Mrs. Williamson,
Burnett Tabrum,
W. B Faraday,
Mrs. Spencer Gollan,
Albert Gray,
Mrs. Albert Gray,

were elected Members.

The Chairman announced that the Managers, at their Meeting held this day, had appointed Dr. Arthur Keith, M.D. F.R.S., Fullerian Professor of Physiology. Dr. Arthur Keith is Conservator of Museum at the Royal College of Surgeons, and Author of "The Human Body," "The Antiquity of Man," and other well-known works.

The Hon. Secretary announced the decease of Mr. Charles Hawksley, M.Inst.C.E., on November 27, 1917, Member of the Royal Institution; and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution, and World of Science, by the decease of Charles Hawksley, Past President of the Institution of Civil Engineers.

Mr. Hawksley was a Member of the Royal Institution for thirty-nine years, and discharged the official duties of a Visitor, Manager and Vice-President. He took a great interest in the affairs of the Institution, and was a generous contributor towards the funds of the Royal Institution.

RESOLVED, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Agricultural Journal, Vol. XII. Part 4. 8vo. 1917.

Geological Survey of India: Records, Vol. XLVIII. Parts 1–2. 1917. 8vo. Memoirs, Vol. XLII. Part 2. 1917. 8vo.

Pusa: Memoirs of the Department of Agriculture, Entomological Series, Vol. V. No. 4. 8vo. 1917.

Accountants, Association of—Journal for Nov. 1917. 8vo.

Admiralty, Lords Commissioners of the—Nautical Almanac for 1920. 8vo. 1917.

American Chemical Society—Journal of Industrial and Engineering Chemistry for Nov. 1917 8vo.

American Geographical Society—Geographical Review for Nov. 1917. 8vo.

American Journal of Physiology—Vol. XLIV. No. 4. 8vo. 1917.

Astronomer Royal—Report of H.M. Astronomer at Cape of Good Hope for Year 1916. 4to. 1917.

Astronomical Society, Royal—Memoirs, Vol. LXII. Part 2. 4to. 1917.

Boston Public Library—Bulletin, Third Series, Vol. X. No. 3. 8vo. 1917.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXV. No. 1. 8vo. 1917.

British Astronomical Association—Journal, Vol. XXVIII. No. 1. 8vo. 1917.

Chemical Industry, Society of—Journal, Vol. XXXVI. Nos. 20–21. 8vo. 1917.

Chemical Society—Journal for Nov. 1917. 8vo.

Chile, Central Meteorological Institute—Publication No. 20. 8vo. 1917.
Observations, No. 21. 8vo. 1917.

Church, Lady—Booklet of Biographical Notices of the late Sir Arthur H. Church. 8vo. 1917.

Davenall, Thos. W. E., Esq., M.R.I.—Drolls from Shadowland. By J. H. Pearce. 8vo. 1893.
Tales of the Masque. By J. H. Pearce. 8vo. 1894.
The Delectable Duchy. By Sir A. T. Quiller-Couch. 8vo.
The Astonishing History of Troy Town. By Sir A. T. Quiller-Couch. 8vo. 1888.
A Week at the Land's End. By J. T. Blight. 8vo. 1893.
Cornish Ballads and other Poems. By R. S. Hawker. 8vo. 1904.
Footprints of Former Men in Far Cornwall. By R. S. Hawker. 8vo. 1903.
The Cornish Calendar, 1901.
The Cornish Magazine, Vols. I. & II. By Sir A. T. Quiller-Couch. 1898–99. 8vo.

East India Association—Journal, New Series, Vol. VIII. No. 4. 8vo. 1917.

Editors—Athenæum for Nov. 1917. 4to.
Author for Nov. 1917. 8vo.
Chemical News for Nov. 1917. 4to.
Chemist and Druggist for Nov. 1917. 8vo.
Church Gazette for Nov. 1917. 8vo.
Concrete for Nov. 1917. 8vo.
Dyer and Calico Printer for Nov 1917. 4to.
Electrical Engineering for Nov. 1917. 4to.
Electrical Industries for Nov. 1917. 4to.
Electrical Times for Nov. 1917. 4to.
Electricity for Nov. 1917. 8vo.
Engineer for Nov. 1917. fol.
Engineering for Nov. 1917. fol.
General Electric Review for Oct. 1917. 8vo.
Horological Journal for Nov. 1917. 8vo.
Illuminating Engineer for Sept. 1917. 8vo.
Journal of the British Dental Association for Nov. 1917. 8vo.
Journal of Physical Chemistry for Nov. 1917. 8vo.
Junior Mechanics for Nov. 1917. 8vo.
Law Journal for Nov. 1917. 8vo.
Model Engineer for Nov. 1917. 8vo.
Musical Times for Nov. 1917. 8vo.
Nature for Nov. 1917. 4to.
New Church Magazine for Nov. 1917. 8vo.
Nuovo Cimento for July 1917. 8vo.
Page's Weekly for Nov. 1917. 8vo.

Editors—continued
Physical Review for Oct. 1917. 8vo.
Power for Nov. 1917. 8vo.
Power-User for Nov. 1917. 8vo.
Science Abstracts for Nov. 1917. 8vo.
Tchèque, La Nation, for Nov. 1917. 8vo.
War and Peace for Nov. 1917. 8vo.
Wireless World for Nov 1917. 8vo.
Zoophilist for Nov. 1917. 8vo.
Florence, Biblioteca Nazionale Centrale—Bollettino for Oct. 1917. 8vo.
Franklin Institute—Journal, Nov. 1917. 8vo.
Geological Society of London—Abstracts of Proceedings, No. 1011-1012. 8vo. 1917.
Imperial Institute—Bulletin, April-June 1917. 8vo.
Linnean Society—Proceedings, Oct. 1917. 8vo.
List of Members, 1917-18. 8vo.
London County Council—Gazette for Nov. 1917. 4to..
Meteorological Office—Weekly Weather Reports for Nov. 1917. 4to.
Daily Readings for Sept. 1917. 4to.
Monthly Weather Reports for Nov. 1917. 4to.
Réseau Mondial 1911-1912. 4to. 1916-1917.
Mexico, Sociedad Cientifica "Antonio Alzate"—Memorias, Tome XXXVI. Nos. 1-2. 8vo. 1917.
New Zealand, High Commissioner for—Patent Office Journal, Oct. 1917. 8vo.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for Sept.-Oct. 1917. 4to.
Pharmaceutical Society of Great Britain—Journal for Nov. 1917. 8vo.
Photographic Society, Royal—Journal, Vol. LVII. No. 7. 8vo. 1917.
Rome, Ministry of Public Works—Giornale del Genio Civile, Aug.-Sept. 1917. 8vo.
Royal Colonial Institute—United Empire, Vol. VIII. No. 11. 8vo. 1917.
Royal Engineers' Institute—Journal, Vol. XXVI. No. 6. 8vo. 1917.
Royal Society of Arts—Journal for Nov. 1917. 8vo.
Royal Society of London—Proceedings: A, Vol. XCIV. No. 656. 8vo.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXIII. No. 11. 8vo. 1917.
Selborne Society—Selborne Magazine for Nov. 1917. 8vo.
Società degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. VI. Disp. 9-10. 4to. 1917.
Spielmann, Sir Isidore (the Author)—The Germans as Others See Them. 8vo. 1917.
Tôhoku Imperial University, Sendai, Japan—Science Reports, 1st Series, Vol. VI. No. 3. 8vo. 1917.
United Service Institution, Royal—Journal for Nov. 1917. 8vo.
United States Department of Agriculture—Journal of Agricultural Research, Vol. XI. Nos. 1-4. 8vo. 1917.
Experiment Station Record, Vol. XXXVII. No. 4. 8vo. 1917.
Washington, National Academy of Sciences—Proceedings, Vol. III. No. 10. 8vo. 1916.
Western Australia, Agent-General—Geological Survey Bulletins, Nos. 60, 63, 66. 8vo. 1915-1916.

WEEKLY EVENING MEETING,

Friday, June 8, 1917.

THE HON. SIR CHARLES PARSONS, K.C.B. J.P. Sc.D. LL.D. F.R.S.,
Vice-President, in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.Sc. Pres.R.S.
Professor of Natural Philosophy, R.I., Master of Trinity.

Industrial Applications of Electrons.

IF there are among my audience any who twenty years ago listened to the announcement I made here of the existence of electrons, they will, I think, admit that they would admit that they would have been very sceptical if they had been told they would in another twenty years be listening to another discourse on the commercial applications of these electrons. For electrons are so small that it takes about 1700 of them to give a mass equal to that of an atom of hydrogen, and they move at such a rate that they get out of the field of operations in a small fraction of a millionth of a second. Such properties appear rather transcendental, and not promising from a practical point of view. But, as a matter of fact, electrons were now not only in trade circulars, but also in the Law Courts.

Yet electrons possessed properties which gave great promise for useful application. Inventors would appreciate the importance of having a body with practically no mass, and yet capable of being acted upon by forces. Again, the electrons existed for exceedingly brief periods, and were independent of what had happened before, or what might happen afterwards. Thus they gave really instantaneous photographs, and not averages over periods comparable with even thousandths of a second. For particles existing only for a hundredth-millionth of a second, the period of an electric wave was almost an eternity, and the particles would register the periodical forces in such waves as readily as if they were steady. This may, I think, lead to a convenient method of registering electric waves. Let the stream of cathode particles pass through the electric field between two parallel plates and connect these plates to an antenna. When no waves were coming along, the spot of light marking the place where the cathode rays struck the phosphorescent screen would remain fixed, but when the waves came, it would be drawn out into a continuous line. It was easy to see that if the incident wave were cut up into a series of dots and dashes it would affect the nature of the trace made by the cathode rays.

The use of electrons in "hot wire valves" to detect electrical waves is now very general, and hundreds of thousands of such valves are now in use for this purpose.

In these valves a stream of electrons is liberated from a cathode of tungsten wire heated by an electric current to incandescence. The principle by which these valves act as detectors of electric waves, in which the electric force is first in one direction and then in the opposite, will be understood if we consider the relation between the current through such a tube and the potential difference between the terminals. This relation is shown by the graphs in Fig. 1, which represent the result of experiments made years ago in the Cavendish Laboratory. The current at first increases with the voltage, but then bent over so that for a certain range the current was independent of the voltage. Suppose now we apply to the tube a voltage just above that corresponding to the beginning of the "knee," then a diminution in the voltage will produce a great diminution in the current, while an increase in the voltage would not affect the current. Thus when electric waves fell on such a valve the positive part would not increase the current, while the negative would diminish it; thus every time an electric wave fell on the valve, the current would be

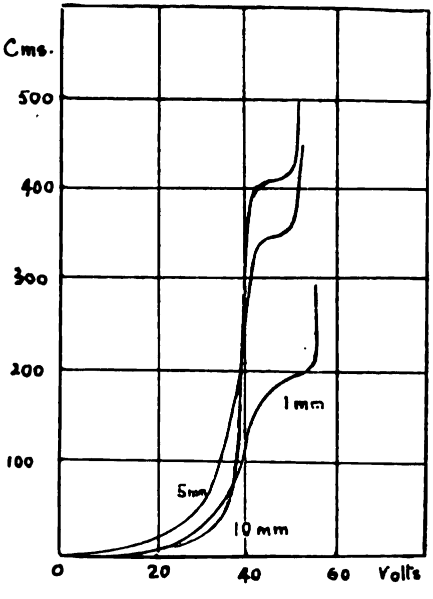

FIG. 1.

diminished. If, then, the electric waves were cut up into dots and dashes, the current would experience corresponding variations in the current; this method was used years ago by Professor Fleming to detect radio-telegraphic messages.

Of late years great attention had been devoted to the improvement of detectors of this type. It is desirable to distinguish two cases:—

1. When the vacuum in the valve is so good that the effect of the residual gas can be neglected, and the discharge is purely electronic.

2. When the conization in the residual gas is appreciable.

In the purely electronic discharge the relation between the current and voltage is very interesting, and has been investigated with great success by Mr. Langmuir. The results are represented by the curves in Fig. 2; the different curves corresponding to different temperatures of the cathode.

The current increases with the voltage up to a certain point and there becomes constant and has a value which depends only on the temperature of the cathode and not upon the voltage; the higher the temperature, the greater the range of voltage where the current increases with the voltage, and the higher also the limiting current. The sensitiveness of the arrangement depends upon the steepness of the curve just before they become flat. This steepness can be increased by the admission of a little gas when we get valves of the second type. The action of the gas may be explained in the following way. The cathode C emits electrons which are driven towards the anode A, some of these electrons accumulate, however, on or near the grid, their repulsion on the electrons coming from C would tend to neutralize the attraction of the anode and the emission of the electrons would be retarded. If, however, a little gas is admitted into the tube, a few positive ions would be produced between the anode A and the grid G, they would move slowly up to the cathode through the perforations in the grid, and would neutralise the repulsion exerted by the electrons; the effect would be that more electrons would come from the cathode and the curve representing the current (shown by the dotted line in Fig. 3) would rise more steeply.

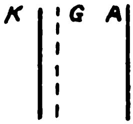

FIG. 2.

I have investigated the theory of this arrangement, and find that the valve is most sensitive when—

$$\frac{\text{potential between grid and cathode}}{\text{potential between anode and cathode}} = \frac{4}{n^2}\,\frac{m}{M}$$

where n is the number of positive ions produced by one electron, m the mass of an electron, M that of a positive ion. As m/M is very small, we see that unless n is exceedingly small, the grid potential ought to be a very small fraction of the anode potential. Another method of obtaining great sensitiveness is to work with gas at a much higher pressure, when the current is due chiefly to conization. In this case I find that in the most sensitive state—

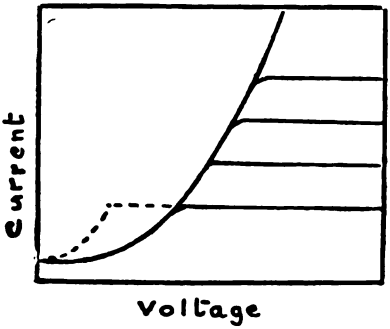

FIG. 3.

$$\frac{\delta c}{c} = \frac{1}{1\cdot 8} \frac{\delta e}{Q}$$

where c is the current, δc the change in c due to the change δe in the grid potential, and Q the conising potential of the gas. The use of these higher pressures is subject to the disadvantage that owing to the action of the luminous gas in the tube, the life of the filament is short.

[J. J. T.]

WEEKLY EVENING MEETING,

Friday, January 19, 1917.

His Grace the Duke of Northumberland, K.G. D.C.L. F.R.S.,
President, in the Chair.

Professor Sir James Dewar, LL.D. D.Sc. F.R.S.,
Fullerian Professor of Chemistry.

Soap Bubbles of Long Duration.

An investigation of the properties of Soap Bubbles is the logical outcome of the experiments on flat liquid films described in the 1916 Friday Evening Discourse. Some fifty years ago Joseph Plateau—Professor of Physics at Ghent—began a series of investigations on Surface Tension Phenomena, and for nearly forty years carried out a number of wonderful investigations, although practically blind, owing to an accident which occurred while examining the action of intense light on the retina in the early part of his career. It is due to the fascinating character of his pioneer work on soap bubbles that the study has been continued. Since the work of Plateau great advances have been made in the experimental study of Surface Tension Phenomena by Dupré, Mensbrugghe, Reinold and Rücker, Michelson and his collaborators. The theoretical advancement which science owes to Willard Gibbs dominates all modern research on capillary phenomena.

The flat films described last year were developed in an exhausted atmosphere of nearly pure water vapour; while soap bubbles are blown up with air in air. Experiments extending over a year have proved that in order to keep soap bubbles for any length of time the air must be free from deteriorating gases and suspended solid matters. The presence of living organisms and their spores amongst the finely divided organic and inorganic particles in the atmosphere was discovered by Pasteur. He purified the air by passing it through a red-hot platinum tube containing platinum gauze, afterwards removing the total carbonic acid. Later Tyndall used a concentrated beam of light to demonstrate the presence of floating impurities in the air, as a test of "optical emptiness." Tyndall passed a parallel beam from an arc lamp through a large plano-convex lens, which condenses the light into a cone, at the apex of which the intensity of the illumination causes the floating matter in the air to be easily recognised. If a Bunsen or hydrogen flame is placed below the focus, the reflected light gradually disappears as the floating material, being largely organic, is burnt and passes into the gaseous state. Another method used by Tyndall to obtain a dust-free space was by smearing the interior surface of a glass box with glycerine, closing and allowing it to stand for a few days, when the dust settled, adhering to the glycerine. On applying the light beam test the box was found to be what Tyndall termed "optically empty." Aitken has devised an apparatus for counting the particles in the air, suggested by experiments on cloud condensation due to the cold produced by sudden expansion of air saturated with water vapour. Very briefly his method is to make these solid particles the nuclei of small raindrops, which can then be counted. Ordinary London air contains about 100,000 suspended particles, organic and inorganic, per cubic centimetre. The particles get coated with all kinds of adherent organic matter which, coming in contact with the bubble, affects its surface tension locally, thereby inducing instability.

When bubbles blown with pure air were formed in vessels closed to prevent communication with the outside air, it was soon recognised that, when the interior of the vessel had become free from suspended matter by long standing and deposition on the walls the surface of which was coated with glycerine soap solution, the bubbles lasted much longer.

The methods of purification employed for the purpose of obtaining long-lived soap bubbles were (1) simple displacement by purified air; (2) deposition of the solid material by electrical discharge in the space to be utilised. To demonstrate the two methods, domed glass shades about 2 ft. high were used, each with a black wood base, in which was a circular glass window. A concentrated beam of light from an arc lamp was reflected up through the window, thus strongly illuminating the solid impurities in the air-space enclosed by the shade. Highly compressed air from an ordinary steel bottle, purified by passing it through three small glass towers containing, respectively, soda, lime, and cotton-wool damped with glycerine, was introduced in the bottom of the shade through holes in a circle of lead pipe secured on its wooden base. In about a minute the beam disappeared as the impure air was swept out through a small hole in the top of the shade. To show the electrical method of purification a similarly fitted shade had an insulated steel point hung within it from the centre of the dome. This was connected to one pole of a Wimshurst machine, while the other was earthed along with the lead pipe described above. Within two minutes of starting the point-discharge the illuminated cloud had practically disappeared. When this treatment was applied to a vessel containing soap solution, a heavy mist of large floating water particles first appeared, of some persistence.

BUBBLES FROM THE EVAPORATION OF LIQUID AIR.

The evaporation of liquid air readily affords a supply of pure mixed oxygen and nitrogen. Apparatus for this purpose is shown in

Fig 1. The liquid air is contained in a tube A, which is fitted by a ground cap to the delivery tube B, and placed in a vacuum vessel D to control heat influx. At B one branch tube leads to the thistle-shaped funnel from which the bubble is to be suspended, while another branch leads to the stop-cock C, by means of which the admission of air into the bubble can be regulated. The cavity above the ground cap, and the horizontal tube leading to the bubble are packed with copper-wire gauze in order to warm the air evaporated.

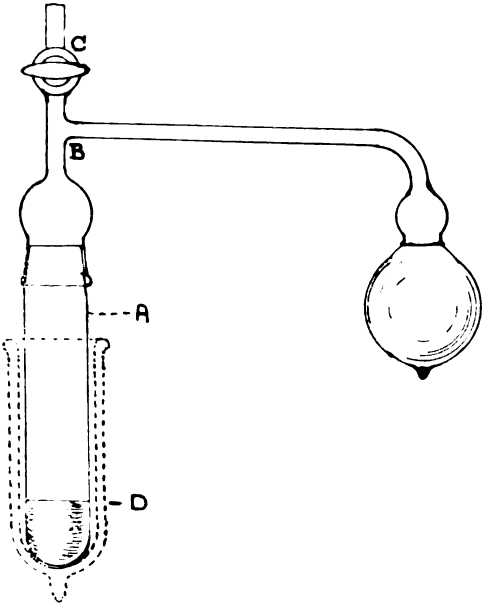

FIG. 1.

The soap solution is supplied to the mouth of the thistle-shaped funnel in the usual way: or otherwise, by having a constriction in the tube above the thistle-shaped funnel, together with a stoppered dropping funnel. It is thus possible to fill the constriction with soap solution, and develop bubbles singly or in succession, as desired.

APPARATUS FOR THE PRODUCTION OF BUBBLES UNDER VARIOUS PRESSURES.

The apparatus, Fig. 2, is a spherical glass flask A of 2½ litres capacity, fitted with an india-rubber cork which has been previously cut and steamed in order to remove volatile organic matter. The tube B leads from the vessel A to a mercury U-tube manometer, and to the stop-cock to which the air-pump is attached; while another tube carries at its lower end the constriction C and the funnel on which the bubble is to be formed, and at the upper end the small reservoir G of soap solution. Air passing through the purifying bulbs F is admitted by the regulating stop-cock D, through the reservoir G to the funnel-mouth C. A capillary tube between D and G assists in the control of air when the pressure in A is very small. By slightly tilting a few drops of the solution in G down the tube to C, bubbles are easily blown within the flask A, even at a pressure less than 1 mm. Hg. At this pressure the contraction of the bubble proceeded visibly, and was accelerated as the diameter was reduced.

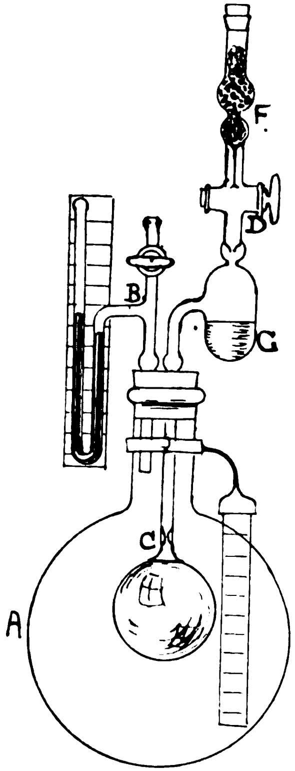

Fig. 2.

In Fig. 3 various forms of bubble-support tubes are shown, convenient for different purposes: (*a*) permits the insertion of a long fine tube or rod into the bubble; (*b*) allows the apparatus to be inverted, without loss of soap solution, so as to have the bubble supported from below: (*c*) permits the formation of bubbles in otherwise inaccessible places (this form has a dropping funnel and stop-cock attachment to the reservoir); (*d*) is useful for forming a succession of bubbles (by a series of slight regulated jerks of the reservoir, a series of films form in its exit-tube, which pass down to the thistle-shaped funnel, and there produce a chain of bubbles).

An ordinary tubulated aspirator, of about 12 litres capacity, is very convenient for the study of bubbles up to nearly 20 cm. in diameter. For careful measurements a plane-sided vessel is necessary to prevent optical distortion. For this purpose a metal-framed plate glass box, 1 foot length of side, was used. The sheet of glass forming the top was nearly 6 mm. thick; it was pierced with three or four holes from ½ cm. to 3 cm. diameter, besides the central hole, 3 cm. in diameter, for supporting the blowing tube. These extra holes allow the

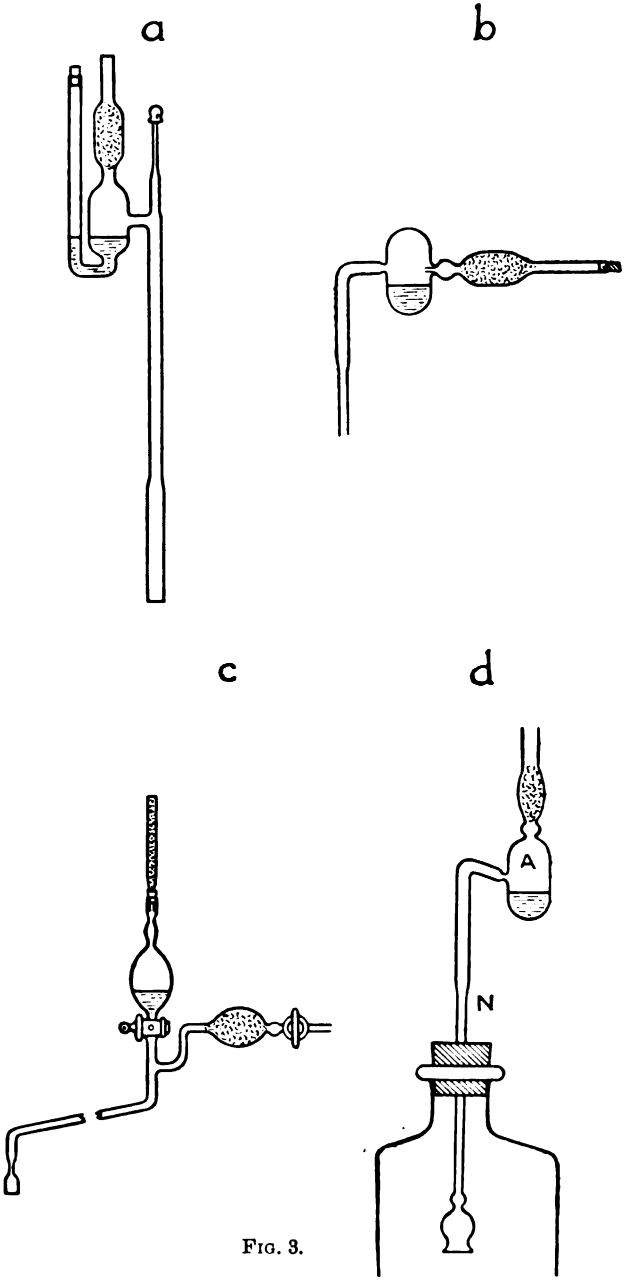

FIG. 3.

admission of flexible glass supporting rings, or draining points, where necessary. Thus, by means of the dropper, Fig. 3*a*, a fine glass thread or wire can be passed down through the bubble, so that the drop is removed and the distortion observed. Similarly, through one of the openings in the lid of the cubical glass chamber, a thin glass rod with an up-turned end, approaching the bubble from below, can be employed to drain off any excess of liquid, and thereafter to determine the changes effected in the vertical and horizontal diameters.

DISTORTION OF BUBBLES, HANGING OR SUPPORTED.

The following instances show the amount of distortion or ellipticity, present in hanging bubbles, under different conditions :—

A bubble, 19·1 cm. vertical diameter, 17·8 cm. horizontal diameter (ellipticity 7 per cent.), when freshly blown from solution of 5 per cent. potassium oleate, 50 per cent. glycerine, was only feebly coloured, and therefore fairly thick. On the fourth day its vertical diameter was 18·1 cm., and on fifth day 17·7 cm., being then sufficiently thinned to be well coloured.

A bubble of 5 per cent. ammonium oleate in 50 per cent. glycerine, measuring 17·25 cm. vertically and 16·2 cm. horizontally (ellipticity 6 per cent.), was at first only feebly coloured in the lower one-fourth. By the third day the vertical diameter was reduced to 16·6 cm. from the reduction of mass, shown by an intensification of the colouring. A bubble of this composition exposed to water vapour, by running water into the containing vessel, will condense and absorb the water vapour and become thickened again for a time. Such a bubble of 30 cm. vertical diameter was dragged down by the water it absorbed in one day to 31·8 cm., an elongation of 6 per cent.

A bubble, 11·2 cm. diameter, with the upper half black and lower half coloured, was reduced to 10 cm. diameter, when entirely black, by drainage—i.e. a distortion of 12 per cent. vertically was caused by the lower half of a thin black bubble thickening to show colour by adding a few drops of soap solution.

If instead of the bubble being supported from above it rests on a suitable light ring support, it will obviously decrease in vertical diameter when it becomes thickened and weighted. This was frequently observed with bubbles of about 40 cm. diameter, resting on a 10 to 12-cm. ring of thin iron wire.

With a water layer in the globe, considerable variations were noted from day to day, e.g.—

Thus, a bubble coloured green and carmine, very brilliant but fairly thick, 42·6 cm. diameter, thinned to half black and half silvery (next thicker colour to black), 43·6 cm. diameter.

Another bubble coloured amber to purple, near lower limit of thickness to show colour, 22·7 cm. diameter, thinned to all black (thinnest possible), 23 cm. diameter.

A coloured bubble 43·3 cm. diameter, thickened in 15 minutes by condensation to 42·7 cm. diameter.

Bubbles of such a size flattened down on their supporting rings as they became thicker and heavier, and could be seen hanging over the ring-support from this cause.

A bubble 39 cm. diameter, hanging from a 4-cm. glass funnel support, thinned to over 70 per cent. black on its second day, and increased until its 63rd day, when it burst, 80 per. cent. black. On five occasions a slowly accumulated drop was observed to fall off —namely, on the 6th, 10th, 20th, 27th, and 52nd days respectively. The alterations in diameter which thereupon resulted were as follow :—

With drop .	39·0	39·05	38·95	38·45	37·80 cm.
Drop fallen .	38·5	38·55	38·40	37·90	36·95 cm.
Alteration .	0·5	0·50	0·55	0·55	0·85 cm.

This bubble was blown from a solution containing 10 per cent. ammonium oleate and 50 per cent. of glycerine.

A bubble of similar size, but resting on a ring, varied in vertical diameter as follows :—

42·6 cm. when fairly thick, at first.
43·5 „ 15th day.
43·0 „ 16th day, orange to amber.
43·3 „ 17th day, 80 per cent. black.
43·0 „ 18th day, four-fifths deep orange, one-fifth purple ;

thus rising higher on its ring-support as it became thinner.

The maximum amount of distortion by loading which a bubble of this size can maintain was observed in one case by expanding the bubble until the distortion at the rim-support, 4 cm. in diameter, showed that it was nearly being dragged off. The dimensions were then 45·4 cm. vertically and 40·1 cm. horizontally (ellipticity 11·7 per cent.). The diminution in vertical diameter by the release of a single drop was 2·1 cm., showing the near approach to instability. After fourteen days the drainage reduced the loading of the bubble so that the vertical diameter became substantially constant, as shown by the following records :—

Day . .	7th.	14th.	21st.
Diameter .	44·3	41·6	41·8

After this date the bubble remained coloured in the lower three-fourths of its area. In this condition the distortion caused by one drop was approximately 1 cm. The persistence of the coloured stage could be maintained by a very minute continuous leak of solution from the reservoir into the supporting nozzle. When the soap solution supply was stopped the development continued to the black state. When completely black the whole mass, in a bubble 40 cm. in diameter, is of the order of 50 mgms., so that the bubbles are very susceptible to disturbance from shock to the containing vessel. Local or sudden changes of temperature must also be avoided, as strong convection currents are thereby set up. Such currents become evident by innumerable silvery discs (see p. 19) streaming continuously in many directions over the black film. The thicker silvery discs floating for a time on the thinner black film represent excess of liquid drawn in either from the drop below or the ring of liquid, at the contact of the bubble with the supporting nozzle. By the use of closed vessels the nozzle is kept constantly moist, and this is probably a factor in the preservation of black bubbles.

DURATION OF LIFE OF VARIOUS BUBBLES.

The records of a large number of bubbles, up to 46 cm. in diameter, have been preserved. Some extracts are condensed in the following summary:—

TABLE 1.—LIVES OF VARIOUS BUBBLES.*

Three, 16–18 cm., 50 per cent. glycerine, 5 per cent. soap, water in vessel; temp. 10°–20° C.	28–42 days.
One, 14 cm., 50 per cent. glycerine, 1[illegible] per cent. soap, water in vessel; half black at end; temp. 4°–11° C.	98 ,,
One, 20 cm., 50 per cent. glycerine, 5 per cent. soap; temp. 9°–11° C.; over 60 per cent. black at end	95 ,,
One, 12 cm., 30 per cent. glycerine, 3 per cent. soap; temp. 10°–20° C.	30 ,,
Black horizontal film, 20 cm. diam., 50 per cent. glycerine, 5 per cent. soap; temp. 4°–20° C.	over 1 year.

TABLE 2.—LIFE OF SUCCESSIVE BUBBLES. 40 cm. Bubbles on Ring.

1st 200-litre vessel.		2nd 200-litre vessel.
1st bubble (vessel dry) . .	3 days	1st, 5 days
2nd bubble (water in vessel)	7 ,,	2nd, few hours (ring rusty)
3rd bubble (water in vessel)	10 ,,	3rd, 3 days
4th bubble (water in vessel)	24 ,,	4th, 18 days
5th bubble (water in vessel, vibration) . . .	19 ,,	5th, 11 days (hot weather, &c.)
6th bubble (water in vessel)	55 ,,	6th, 63 days (hanging on 4 cm. support)
7th bubble (water in vessel, out in yard, frosty at end)	31 ,,	7th, 42 days (globe became contaminated)

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* A hanging bubble kept in air saturated with water vapour, of an initial capacity of 4·5 litres, took 322 days to completely collapse from air transfusion.

The first item of Table 1 includes the first bubble (50 per cent. glycerine, 5 per cent. ammonium oleate), which kept its colour instead of developing to blackness. This was caused by moisture left after cleaning the 5-litre bottle in which it was blown; no more moisture than enough to give a bedewed appearance was left, but this distilled to different parts of the vessel, as the local temperature altered, and resulted in a movement of water vapour sufficient to keep the absorbent bubble thick enough to show colours.

Sharply marked zones of different colours are a feature of such bubbles. For this an undisturbed atmosphere is necessary; otherwise convection currents in the bubble will, by continual mixing, prevent the quiet development of separate colour zones. The temperature alterations must, therefore, be small. From the records of one bubble the following appearances were noted:—

On the 15th day. 20 cm. in diameter: sharp boundary line at "60° N. Lat." between steel-blue above and greenish-yellow below: similar boundary between blue-green and blue-purple at "50° S. Lat." Large drop on bubble.

On the 18th day. Diameter, 19·4 cm.: three sharp line boundaries: green to purple at "30° N.": dark green to light green at "70° S.": light green to thin magenta at "80° S.": intense green disc at lowest part. Drop fallen off.

On the 24th day. Main area of bubble fairly uniform red-purple: boundary at "75° N." to pale green above, and to deep green below at '60° S." Graded Newton's rings of mauve and green below this.

The life of the bubble shown in the second item of Table 1 was double that of those in the first item, namely, 98 days instead of about 40 days. The solution used had 10 per cent. of ammonium oleate instead of 5 per cent. as before. This factor, however, had less influence than the generally lower temperature of under 11° C. instead of up to 20° C. as before. Between the 75th and 95th days there was a contraction from 11·4 cm. diameter to 10·4 cm., while still remaining coloured. By the 95th day the bubble had become too dilute to maintain any colour, and accordingly developed to the thinnest or "black" stage in the upper half. On the night of the 98th day the temperature of the Laboratory fell to near freezing point, and the bubble did not survive this change.

The next item refers to a bubble of 5 per cent. potassium oleate in 50 per cent. glycerine. The 12-litre aspirator in which it was blown was well dried, and, further, was placed in a vault where only slow and small temperature variation occurs. In three months the variation was from 9° C. to 11° C. This solution was chosen because of its property of giving extremely slow development to the "black" stage. The result was that in 95 days the zone of black had only extended down to "25° S. Lat.," the remainder being so thick as only to be feebly coloured with faint pink-and-green rings, probably up to a hundred times the thickness of the black supporting zone above. The final collapse may have been associated with this condition; moreover, the potash soap is very sensitive to slight alkalinity of the glass,—no other disturbing cause was observed.

Table 2 shows the lives of two sets of large bubbles under similar conditions, in two vessels of 200-litre capacity (Fig. 4). These vessels were cylindrical glass globes, used in the Laboratory twenty-five years ago in the production of liquid ethylene. They had short necks, 14 cm. diameter, which were fitted with good, cleansed india-rubber corks to carry the blowing tubes, vent tubes, etc. An oxidised steel wire ring was used to support the majority of the bubbles. These rings were about 12 cm. diameter, and were raised, by three supports of the same oxidised wire, to about 25 cm. above the bottom of the globe. For bubbles so supported the blowing tube was less than 1 cm. diameter; it had a constriction of about 1 mm. bore, a few centimetres above the lower end. A few drops of liquid from the reservoir above were decanted down to fill the constriction for the

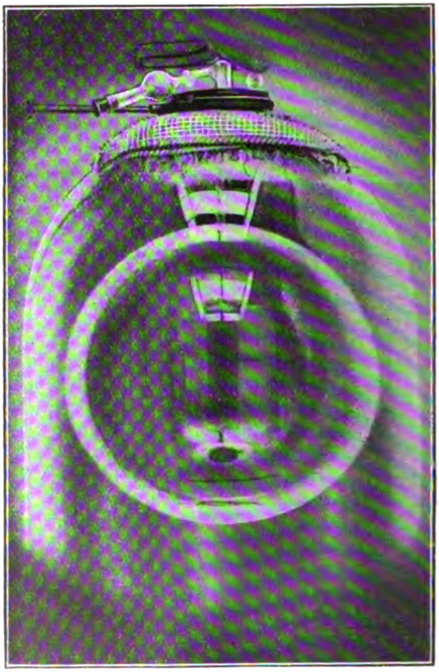

Fig. 4.

purpose of starting the bubble. The reservoir is shut off by a stopcock from the blowing tube, as it is not advantageous for the soap solution to be exposed to the current of air used in expanding the bubble : a side tube is sealed in for the air inlet. The blowing tube is mounted, so that it can slide up and down while remaining air-tight. A few small bubbles are first blown and run round the supporting ring to wet it. A bubble is then blown to about 4 cm. greater diameter than the ring, while resting on one side of it. This is done by holding the end of the blowing tube sufficiently out of the centre, and steadily raising it until the bubble is about 15 cm. diameter, when it is allowed to come into full contact with the whole ring, and the expansion is continued until the requisite diameter of about 40 cm. is reached. Meanwhile, the blowing tube is continually raised through its sliding support tube in the india-rubber cork, and finally withdrawn by a to-and-fro spiral movement upwards. In one case when the bubble became free from the tube, a complex up-and-down oscillation took place in a period of about two seconds, and with an initial amplitude approaching 3 cm. at the upper periphery. By leaving the blowing tube in the bubble the advantage of an added steady support is secured ; this is unsatisfactory when measures of contraction are being made, but it certainly helps to prevent undue disturbance, especially in the early stages, when the bubble is liable to be thick and somewhat top-heavy on the 10 cm. ring.

It can be seen from Table 2 that there is a general increase in length of life of the successive bubbles in both the first and second vessels. The last items, in both cases, show a shorter life ; this, however, was due in one case to the vessel being in the open air in winter, whereby its temperature at the end went below freezing point ; and in the second case, to the globe becoming contaminated, from the displacement of a badly-fitting tube. The irregularity between the fourth and fifth items was explained by the hot weather (temperature over 20° C.).

The maintenance of colour in such large bubbles was not easy. For this purpose a good circulation of water vapour in the vessel seems desirable, as the mere presence of a litre or two of water in it was not entirely effective. A wet cloth placed on the upper part of the vessel (seen in Fig. 4) by its own evaporation maintained a slightly cooler area, thus promoting the condensation there of drops of water. A flow of water vapour up through the vessel was thus obtained, which being partly absorbed by the glycerine in the bubble did for a time maintain, and even increase, its thickness and colour. In the later stages also, when the bubble material has thus become altered in the relative proportions of its constituents, vaporisation from the bubble may take place, and then the opposite effect is observed; distillation causing the thickness to decrease, so that two or three colours are passed through in as many hours. A few degrees rise of room temperature causes the same change, especially in diluted bubbles. The lowering of the temperature will then reverse the effect and the bubble is seen to thicken. The ordinary variations from night to day are sufficient to cause many such repetitions in the same bubble. Thus, when it was thought to increase the proportion of water vapour by warming the water in the vessel, and thereby increasing the thickness by a greater absorption, the rise of temperature counteracted any increased absorption; and once more the bubble became thinner. The removal of the wet cloth allows the condensed water to distil off more vapour, which is often sufficient to thicken the bubble. This is best seen when a good condensed deposit is accumulated before blowing the bubble, as was done in one case (see fifth bubble, first vessel) by the application of bags of ice. The bubble, which was then blown, remained almost entirely coloured throughout its life. A small zone of black came and went, and the thickness was reduced once or twice by a damp cloth. The weather was rather warm at first, but became more temperate (the time was August). Finally, the bubble was very much thickened, and probably burst from this cause; for as the bubble became thick, it overhung the ring unsymmetrically, and most probably became lopsided enough to swing round and touch the vessel. The supporting ring was seen to be slightly out of the horizontal, and in vibration due to the working machinery in the room below.

The fourth bubble in the second vessel: during the first week untouched; maintained green and pink colour; thick and overhanging the ring; then frequently treated by damp cloth above, thinned while cloth was on and thickened when cloth was removed; at third operation, bubble half black; next time, the water below was warmed by the arc lamp beam: result, 85 per cent. black obtained.

The fifth bubble in the second vessel lasted eleven days only: short life in the hot weather; disturbed at the end by damp cloth, also by vibration; thinned to silver and black; first three days thickened rapidly by absorption from the deposit of water; immediately after being blown sagged over ring; then steadily thinned through several beautiful grades, until on the fifth day at 19½ C. it was amber and purple, with silver above. Damp cloth then put on: the silver extended all over, followed by a fairly uniform development of black. Two grades of black were noted—the deepest black being only a zone of a few centimetres at the top. The top of the bubble was approximately 1 cm. higher when black than when thick and heavy.

The sixth bubble in the first vessel went quickly black in two days (possibly from an unusual excess of ammonia in the soap solution), and remained so until the end of the first week. It then thickened to various colours for four weeks, and once again went quickly black. This condition remained for three weeks, during which time a steady diminution of diameter was measured. The blowing-tube having been left attached to the bubble, the contraction had by this time narrowed down the upper part of the bubble till it became a neck. When this divided, the disturbance was probably more than the very dilute and very thin film could survive, for the next morning it had gone.

The seventh bubble in the second vessel was suspended from a 4-cm. nozzle; it lasted from 27th October to 9th December. The temperature fell from 15° C. at first to below 10° C. after the first week. The bubble burst after a sudden rise of temperature from 6° to over 9°, accompanied by a considerable fluctuating fall in the barometer, when it was found, by the optical test, that the air in the globe had become contaminated. This bubble afforded an opportunity for observing the rate of fall of condensation drops. Excluding the preliminary drainage, the results are shown in the annexed diagram (Fig. 5).

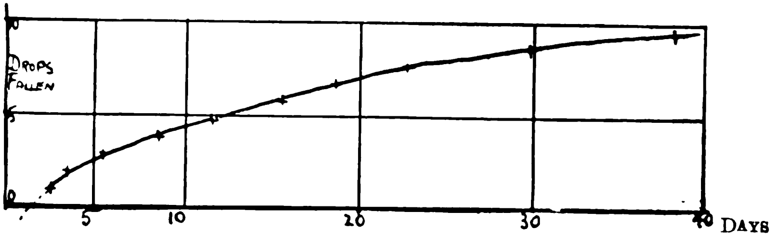

Fig. 5.—Absorption of Water Vapour by a 40 cm. Bubble. Rate of fall of drops.

Gas Transference Through Bubbles at Atmospheric Pressure.

Long-lived bubbles regularly diminish in diameter, and most rapidly when thinned to "black." The contained air or other gas is at a somewhat higher pressure than the atmosphere in which the bubble stands, and therefore tends to pass out; with the result that there is a continual diminution in size. With black bubbles up to about 15 cm. the change becomes very evident in about a week. Using a cathetometer, the contraction can be observed accurately from day to day. The subjoined diagram (Fig. 6) shows the contraction, measured every third day, of a black bubble in hydrogen. From an initial diameter of approximately 11 cm., by the twenty-third day it had completely contracted. It will be noticed that the rate of contraction was accelerated as the diameter decreased.

The gradational diminution in diameter of black bubbles made from soap solutions of different compositions, and with diameters up to 46 cm., was periodically measured with the cathetometer.

The rate of gas transference from within the bubble outwards at any time can be obtained directly from the mean daily reduction in diameter at that time, and is readily measured by dy/dx, the slope of the tangent drawn at the desired point of the contraction curve. The amount of the gas transference, in cubic centimetres per day through unit area, is then equal to one half of the daily rate of reduction in diameter, measured in centimetres.

This follows directly from the simple properties of a sphere; for if S and V be respectively the surface and volume, and D the diameter, then $S = \pi D^2$, and $V = \frac{\pi}{6} D^3$, so that $dV = \frac{\pi}{2} D^2 . dD = \frac{1}{2} S . dD$, therefore dV/S, the rate of gas transference per unit surface, $= \frac{1}{2} . dD$, which is half dy/dx, the daily rate of reduction in diameter shown by the slope of the tangent already referred to.

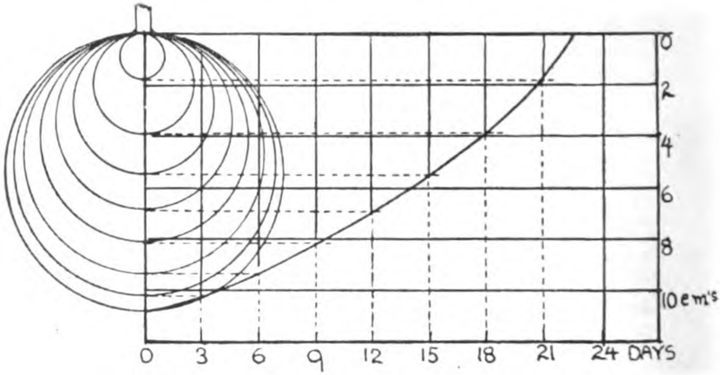

Fig. 6.—Black Bubble in Hydrogen.
Decrease in size at 3-day intervals.

Some of the results obtained are shown in the following Tables 3, 4, 5, and 6, in which are given respectively (1) the diameter of the bubble on which the measures were made; (2) the corresponding internal excess pressure, above the exterior air pressure, given in mm. water; and (3) the measured value of $dD/2$, representing, as shown above, the rate of gas transference in c.c. per day passing out through each square centimetre of the bubble when at the diameter given in line (1).

Gas Transference through Black Air Bubbles.

Table 3.—50 per cent. glycerine, 2½ per cent. soap; hanging on glass tube.

Diameter	6	7	8
Internal pressure .	0·30	0·26	0·23
Gas transference rate	0·065	0·035	0·030

Table 4.—25 per cent. glycerine, 4 per cent. alcohol, 5 per cent. soap; hanging on glass tube.

Diameter . . .	10	12
Internal pressure .	0·18	0·15
Rate	0·15	0·11

Table 5.—Two large black bubbles (thinned by water condensation); hanging on a glass tube.

	(I)	(II)
Diameter . . .	38	38
Internal pressure .	0·057	0·057
Rate	0·023	0·015

The corresponding rates calculated for similar bubbles 10 cm. diameter would be: (1) 0·086; (2) 0·58; whereas a bubble subject to water condensation, but still retaining some colour, contracted from 11 cm. diameter on its 75th day to 10 cm. diameter on the 95th day, being a diminution of 1 cm. in twenty days, giving an average daily rate of gas transference per unit of surface equal to 0·025 cm., or about one-third the calculated rate for a black bubble of the same diameter.

Table 5 may be amplified as follows, to show more completely what is occurring in bubbles of this size:—

Table 6.—Gas transference through 38 cm. black bubbles in air (dilute glycerine-oleate solution).

Results		(I) 5 weeks black*	(II) 2 weeks black †
Diminution in diameter . . .	Daily	0·045 cm.	0·03 cm.
	Total	1·58 cm.	0·41 cm.
Diminution in volume . . .	Daily	0·103 litres	0·067 litres
	Total	3·60 litres	0·940 litres
Gas transference through each sq. cm.	Daily	0.023 c.c.	0·015 c.c.
	Total	0·805 c.c.	0·205 c.c.

* No. (I) hanging on a glass tube. † No. (II) less diluted than No. (I), and resting on a steel wire ring.

In the case of a hanging bubble as large as 38 cm., referred to in Table 6, the total reduction in the diameter amounts only to 1·58 cm. in five weeks. The volume of such a bubble is over 30 litres, and the surface has an area of 5,000 square cm., while the total weight is only about half a gram. Being thinned to the "black" stage, it has a maximum thickness of approximately fifteen $\mu\mu$'s; that is to say, one and two-third million such films if superposed would have a total thickness of one inch. The internal excess pressure is of the order of a twentieth of a millimetre of water, or about one fifteen-thousandth part of the pressure of the atmosphere. Nevertheless the actual volume of air which passed out of the black bubble in five weeks was 3·6 litres, or every minute a layer of air molecules some ten to eleven times the thickness of the wall of a black bubble of 38 cm. diameter passed through it. Hence the thickness of the layer of air which at ordinary temperature passes through the film in one second is 10/60ths of the thickness of the film, or 2·5 $\mu\mu$'s—in other words, a layer of air as thick as the film would take six seconds to pass through it. Again, the mean free path in air is 100 $\mu\mu$'s, which is nearly seven times the thickness of the film, and therefore some forty times the thickness of the layer of air passing through the film in one second. Theoretically the internal pressure

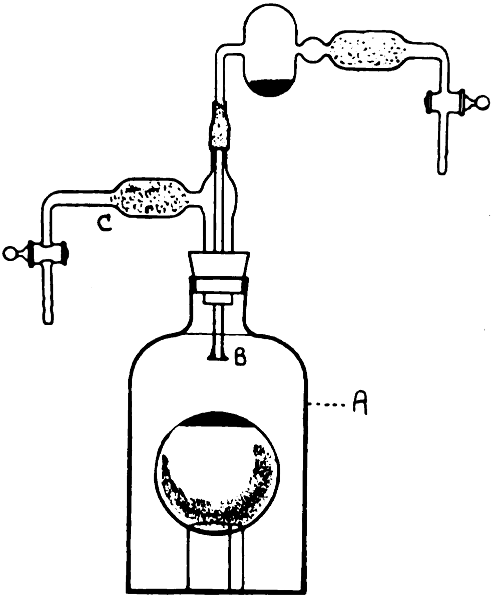

FIG. 7.

in a black bubble of less than about 1 cm. diameter would be sufficient to cause the transference of a layer of gas of a thickness equal to the length of the mean free path. The rate of transference in bubble **II.** is only about three-quarters of that in bubble **I.**

A black spherical air bubble about 42 cm. diameter, supported by a nozzle above and a small fixed horizontal glass ring below: thus keeping its vertical diameter constant, altered to an ovoid shape, 33 cm. horizontal diameter, in 109 days. The lessened internal air pressure caused by the ovoid form reduced the rate of the air transference to about one quarter that in bubble **I.** (Table 6).

Gas Transference through Black Bubbles in Hydrogen.

A few hydrogen bubbles blown in hydrogen were also observed. A suitable vessel for these bubbles is shown in Fig. 7. An oxidised iron-wire ring is seen supporting a bubble, not yet completely black, within the flat-bottomed vessel A (about 3 litres capacity). The filling of the vessel A with hydrogen is attended with considerable difficulty, and requires great care in order to obtain "optical emptiness" in the vessel. The nozzle B should be pushed down nearly to the bottom, while the vessel A is being filled with hydrogen by the tube C (used later as an outlet when expanding the bubble). No Tyndall cone will be visible if the vessel is properly filled. The bubble is then blown on B in the usual way, and expanded and let down to rest on the wetted ring already prepared for it; or it may be left suspended from the nozzle. Both inlet and outlet tubes are protected by glycerined cotton-wool filters.

The contractions observed in hydrogen bulbs were generally more rapid than those of similar-sized air-bubbles, as shown in Tables 7, 8, 9, where the units are the same as in the last Tables.

TABLE 7.—50 per cent. glycerine, 5 per cent. soap.

	Hanging from glass nozzle.				Standing on iron-wire ring		
Diameter . . .	4	6	8	10	6	8	10
Internal pressure .	0·45	0·30	0·23	0·18	0·30	0·23	0·18
Rate of gas transference	0·22	0·14	0·10	0·053	0·253	0·218	0·190

TABLE 8.—33 per cent. glycerine, 3 per cent. soap; hanging from glass nozzles.

Diameter	4	6	8	10
Internal pressure	0·45	0·30	0·23	0·18
Rate of gas transference	0·31	0·25	0·20	0·17

TABLE 9.—Approximately 1 per cent. glycerine-soap; pale golden colour; hanging from glass nozzles.

Diameter	4	6	8
Internal pressure	0·55	0·36	0·28
Rate of gas transference	0·285	0·103	0·050

The internal pressure was not determined for every bubble; the values given above were deduced from results obtained with typical bubbles connected to an alcohol nearly-horizontal displacement manometer. The instrument gave with 1 mm. of water pressure a displacement of the order of 100 mm. of the alcohol column.

A study of these results shows at once that there are obscure factors in some cases, causing notable divergences, even when the conditions are not greatly altered. Other experiments have shown that small differences in composition have in many cases a large effect on the behaviour of these thin films. Table 4 (air bubbles) shows that 4 per cent. of alcohol added to the soap solution increases the rate of gas transference through an air bubble.

The constancy in thickness of the "black stage" is undoubtedly subject to variation in different solutions. Johonnott, using a large number of small black films in a Michelson interferometer, found that there were at least two values for the thickness of the black film he measured, and that the additions of either glycerine or potassium nitrate tended to give the thicker of the two.* In last year's Discourse a description of five distinct grades of black was given, obtained with soap solution containing over 30 per cent. of glycerine. The film covered a thin glass frame in an exhausted glass vessel. These grades were seen to be unstable, coalescing to the deepest or thinnest black when a portion of soap solution was brought in contact with the lower part of the glass frame. It is possible that a few per cent. of alcohol, with its low viscosity and surface tension, might result in a still thinner black stage.

GAS TRANSFERENCE THROUGH BUBBLES AT PRESSURES OTHER THAN ATMOSPHERIC.

A bubble of about 10 cm. diameter, shown in an exhausted flask (Fig. 2), in which the air pressure had been reduced to the order of a fraction of a mm., was seen to contract visibly from the rapid percolation of the air from within outwards. Under such conditions the small internal excess pressure of about 0·2 mm. water, necessary to maintain a 10 cm. bubble, is a very large proportion, say about 10 per cent., of the total air pressure in the flask. There is, therefore, a proportionately small resistance to its percolation through a film sufficiently thin. As the air pressure in the vessel is increased, the time for complete contraction becomes greater, because the internal excess pressure is a relatively smaller proportion of the total opposing pressure; thus when the containing vessel is at atmospheric pressure, the 10 cm. bubble is distended by only $\frac{1}{5000}$th of this total pressure. There is therefore a proportionately small force urging the contained air to pass out compared with that where the vessel was under a pressure of only a fraction of a mm. The result is naturally that the complete contraction takes weeks instead of minutes. In the same way if a bubble be formed in a strong glass vessel charged to, say, 10 atmospheres, then even after six months, during the major part of the time being black, no sensible diminution in diameter was detected.

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* "Thickness of the Black Spot in Liquid Films." By Edwin S. Johonnott, Jun., Ryerston Physical Laboratory, University of Chicago. Phil. Mag., xlvii., p. 501.

The measures of contraction at very low pressures were hindered by the difficulty in securing uniformity of thickness in the film. The black bubbles when obtained are extremely fugitive, as they contract so quickly. Then also the ammonium oleate solutions, which are so well adapted for obtaining black bubbles, do not remain of the same composition under low air pressures, due to the removal of the volatile easily-dissociated ammonia.

The results of some of the attempts with ammonium oleates are given in Fig. 8, showing the contraction at pressures of 1 mm. and 5 mm. Hg. respectively, with approximately the same bubble thickness. The curves in Fig. 9 show the relative rates of contraction with potassium oleate at three different thicknesses of bubble, all at the same pressure of just over 1 mm. Hg.

The first of these two diagrams shows that at less than 1 mm. pressure of air in the vessel, a 14 cm. bubble composed of 3 per cent. ammonium oleate in 30 per cent. glycerine, of intense green coloration, contracts completely in about 17 minutes; whereas if the air pressure be raised to 5 mm., the contraction of a similar bubble has only gone from 14 cm. to 11½ cm. in the same time, and to 10 cm. in 25 minutes. The second diagram shows that a bubble 10 cm. in diameter, composed of 5 per cent. potassium oleate, 50 per cent. glycerine, and almost too thick to show colour, takes 47 hours to contract to 6 cm.; but when a bubble of similar size and composition is thinned to intense green, the same amount of contraction occurs in 9 hours: when further, the black stage is secured, the time for complete collapse from 3 cm. diameter is only 25 minutes. The green bubble, from its graph, would have taken from 2 to 3 hours for the same contraction. Such preliminary results may serve at least to point the way to a more complete inquiry.

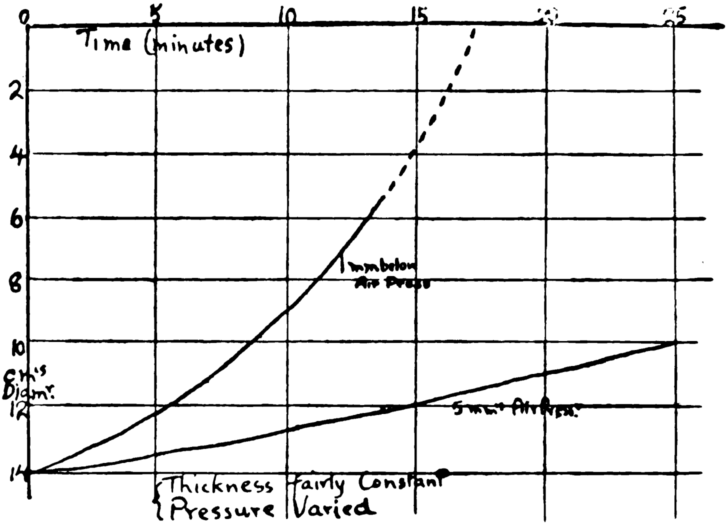

FIG. 8.—BUBBLES OF AMMONIUM OLEATE, 3 PER CENT. IN 30 PER CENT. GLYCERINE. Thinned to first green.

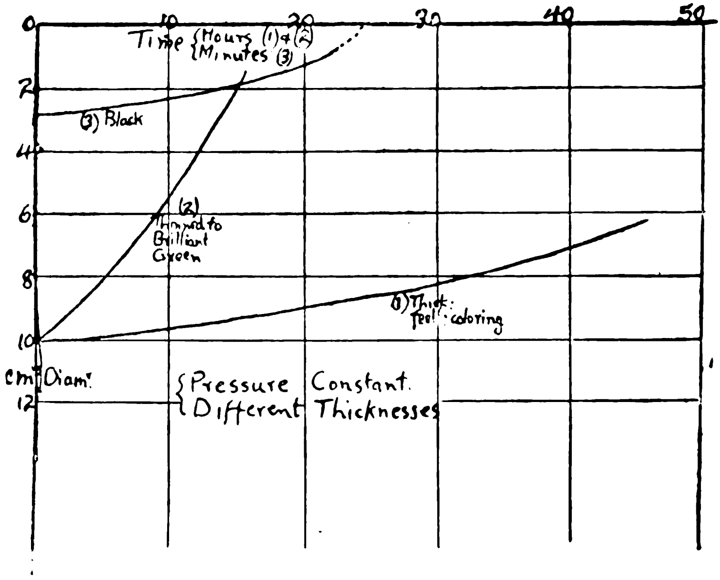

FIG. 9.—BUBBLES OF POTASSIUM OLEATE, 5 PER CENT. IN 50 PER CENT. GLYCERINE. Air pressure over 1 mm. Hg.

EFFECT ON THE GAS TRANSFERENCE RATE OF THE COMPOSITION OF THE SOAP SOLUTION.

The internal excess pressure P, in a bubble of constant composition, and therefore of constant surface tension T, enclosed in a vessel sealed off from the atmosphere, varies inversely as the diameter D, according to the law $P = 4T/D$.

Hence if the rate of gas transference varies directly as the internal excess pressure, it will consequently vary inversely as the diameter. Now, in Fig. 6, taking the origin at the point where the bubble vanishes, and measuring x (time) horizontally to the right in days, and y (diameter) vertically downwards in cms, we found that the gas transference was measured at any time x by $-\frac{dy}{dx}$; hence $-\frac{dy}{dx} = \frac{k}{y}$ where k is a constant, the graph of $\frac{dy}{dx}$ to $\frac{1}{y}$ is a straight line, and the curve of contraction is the parabola $y^2 = D_0^2 - kx$, where D_0 is the initial diameter—a result usually found to be the case. The equation of the particular curve in Fig. 6 is $y^2 = 121 - \frac{121}{23}x$.

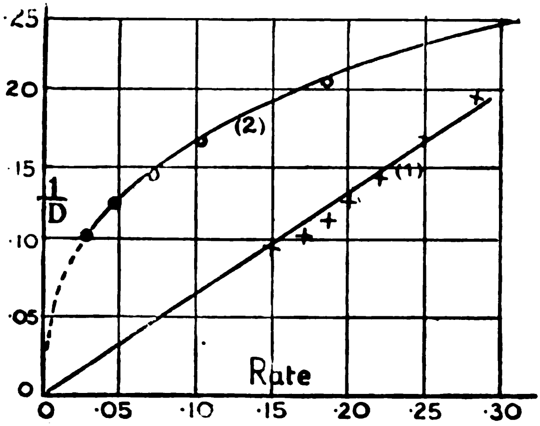

FIG. 10.—GAS TRANSFERENCE THROUGH BUBBLES.

Rates plotted with reciprocals of diameters. (1) Black H bubble of constant composition. (2) Golden H bubble of increasing dilution.

But, when a bubble partly composed of glycerine is exposed to water vapour and thereby progressively diluted, then the surface tension will increase; and the internal pressure will rise at a greater rate than would be due to diminution of diameter alone. The graph of the rates of gas transference, plotted with the reciprocals of the corresponding diameters, would no longer be a straight line. The next figure (Fig. 10) shows the graphs obtained in the case of (1) bubble of constant composition, (2) bubble of glycerine solution subjected to absorption of water vapour. In this second case (see also Table 9) the rate of gas transference $dD/2$ (plotted as abscissæ) is increasing more quickly than the inverse of the corresponding diameters $1/D$ (plotted as ordinates)—a fact which could be explained (1) by an increase in the surface tension, with a resulting proportional increment in the internal pressure of the contracting bubble: and (2) by a more rapid percolation of hydrogen through the diluted film.

It is evident that further comparative experiments are necessary with more dilute films of constant composition before these effects can be differentiated. Preliminary attempts showed that a solution containing only 0·15 per cent. of ammonium oleate with 1·5 per cent. glycerine can give a fairly long-lived bubble (one such in hydrogen contracted completely in 28 days from 8 cm. diameter: see p. 29). But the maintenance of constant thickness is not easy, because with bubbles of such dilution even slight variations of temperature cause recurrent colour changes; it is specially difficult to maintain the black stage.

No great alterations of temperature occurred in any of the above experiments, but distinct evidences of variations from this cause were noted on several occasions. The laws governing this factor have yet to be elucidated.

ALTERATION OF THICKNESS BY CONTRACTION. SILVERY DISCS. CONSTANCY OF BLACK ZONE.

A contracting bubble that remains constant in weight will, of course, become thicker as the surface diminishes. The thickness will vary inversely as the square of the diameter, provided the capillary, viscous, and gravitational forces did not come into play. For small changes, on the simple theory, the rate of logarithmic increase of thickness will be twice the rate of logarithmic diminution of diameter. This is a property of the sphere that may be expressed as follows:—When S = surface, T = thickness, ρ = specific gravity of the liquid composing the bubble, then, since $S = \pi D^2$,

$$\text{Mass} = S\,T\,\rho = \pi D^2 T \rho$$

and as the mass and composition are assumed unaltered, this gives

$$D^2 T = \text{constant, or } T \text{ varies as } \frac{1}{D^2} \quad . \quad . \quad . \quad (1)$$

i.e. the thickness varies inversely as the square of the diameter, and is true for all magnitudes. Differentiating,

$$2\,T\,D.dD + D^2.dT = 0, \qquad \text{or } \frac{dT}{T} = -2\frac{dD}{D} \quad . \quad (2)$$

which can be put in the form

$$d(\log T) = -2\,d(\log D) \quad . \quad . \quad . \quad . \quad (3)$$

Thus the rate of logarithmic increase of the thickness is twice the rate of logarithmic diminution of diameter.

In black bubbles increase of thickness can be observed if the contraction is rapidly effected by opening the closed bubble to the air, or by cautiously removing the air by suction; but the immediate effect is not to produce a coloured bubble. What happens is that small silvery circular discs (see p. 8) break out over the surface of the black bubble, and these represent the aggregations of liquid resulting from the contraction, for they are much thicker than the black film through which they move. They very soon settle to the lower parts, and there slowly coalesce to a small graded coloured area. This usually contracts in a short time to a much smaller zone, too thick to show colour, and finally very little more is left than a small drop surrounded with a narrow silvery band on the otherwise black bubble. Large black bubbles contracting by gas percolation go too slowly for these effects to be observable, but the final result is the same: a drop of liquid slowly collects on the black bubble, and may in large bubbles grow sufficiently to fall off, after causing a progressive elliptical distortion.

Comparable behaviour is shown when one or two drops of solution per minute are allowed to trickle into a black or partly black bubble. Coloured streaks appear and pass downwards from places all round the supporting nozzle into which the liquid is fed; later on the streaks break up into coloured discs, falling slowly by sinuous paths and with diverse motions. However, they all pass through the black portion, and after remaining for several minutes as a mass of distinct many-coloured particles, form by coalescence a coloured zone below round the drop. If the liquid is fed in more rapidly, the coloured zone will rise to a certain extent, but, in single bubbles, a large area of black almost always remains. The feeding of the liquid may be cautiously increased almost to a stream; the bubble is then, of course, much disturbed at the boundary of the black zone, as well as elongated by the loading, and drops fall away in steady succession. The paths through the black zone may then become so thick as to be transparent threads of liquid, without any colour in themselves, but causing coloured rays and patches to flash outwards from their paths.

It was further observed that when a partly coloured bubble quickly altered in size, the black zone remained roughly constant in area, being independent of the extended or contracted surface. The thickness seemed to alter at the expense of the coloured portion, which consequently changed in tint. A bubble is allowed to develop to blackness for about one-third of its surface, the remaining two-thirds grading from a silvery boundary at the black, through golden colour to deep steel-blue. This state is reached in a few hours with a solution composed approximately of 3⅓ per cent. neutral ammonium oleate in 33 per cent. glycerine, with a slight excess of ammonia. Now gradually expand the bubble by a slight reduction of the external pressure. Meanwhile the level of the black boundary as well as the change of diameter are observed. The coloured area is seen to change from steel-blue to blue-purple through magenta and deep amber until almost the whole of it is a full golden colour as a result of the diminished thickness, which however is still of the order of fifteen times that of the black. During this time the black boundary has risen slightly in level; but when the area of the black zone is calculated at successive stages, the value is found to remain constant to within a few per cent. All liquid must be well drained from the hanging bubble by leaving a drainage tube in contact with the drop position, during the preliminary development to partial blackness. Two sets of observations, showing the approximate constancy of the area of the black zone, while the coloured portion is varying to a large ratio, appear in Table 10 below.

Table 10.

Whole Bubble		Black Zone	Coloured Zone	Ratio
Diam. (cm.)	Area (cm.)^2	Area (cm.)^2	Area (cm.)^2	Coloured : Black
(i) 17·55	967·6	90·4	877·2	9·7
15·75	779·2	94·5	684·7	7·3
13·70	589·6	94·7	494·9	5·2
11·60	422·7	91·1	331·6	3·6
9·54	286·0	84·7	201·3	2·4
(ii) 19·1	1146	77·7	1068·3	13·9
16·1	814	76·4	737·6	9·7
13·1	539	74·5	464·5	6·2
10·1	302·5	69·1	233·4	3·4
7·1	158	35·1	122·9	3·5

Thus, from the first four observations, the actual black area of the first bubble only varied some 2 per cent. from its mean value 92·7 cm.^{2}, while the coloured-to-black ratio fell from 9·7 to 3·6, or some 63 per cent.: similarly the actual black area of the second bubble varied about 5 per cent. from its mean value 74·5 cm.^{2}, while the coloured area fell from nearly 14 times to 3½ times the black area—a drop of over 75 per cent.

Absorption of Water by Bubbles Composed of Oleate Glycerine Solution.

When boiled water is introduced into a vessel which holds a bubble composed partly of glycerine, water vapour is absorbed and the normal thinning of the bubble to the black stage is prevented. There is instead an increase in thickness, causing gradational changes of colour: the bubble sags down from the increase in weight; successive drops accumulate and fall off, at first, maybe, two or three in a day, decreasing rapidly in number as the dilution proceeds, until when the soap-glycerine is reduced to 1 or 2 per cent. the interval between the drops grows into many weeks, and the drainage practically ceases. When very diluted the black stage is finally reached, and may remain, provided the temperature is steady; but usually much fluctuation of colour takes place, the black even recurring many times.

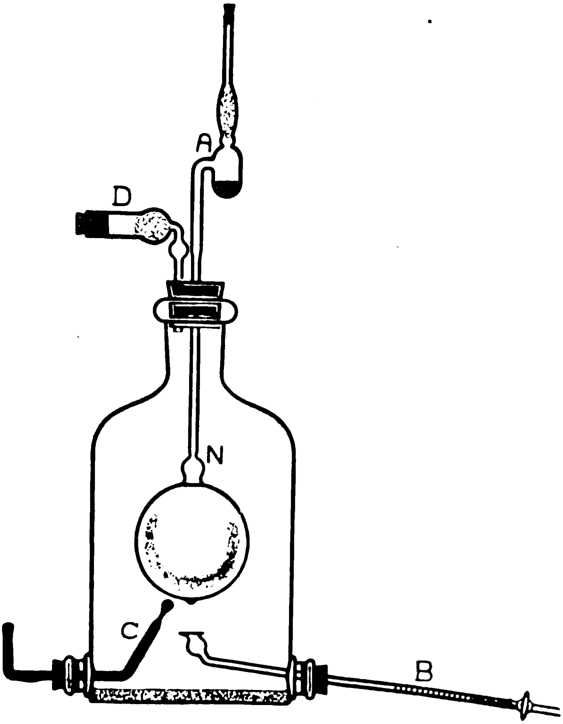

Fig. 11.

The washings of several such bubbles were collected in a graduated tube, and periodically weighed and analysed. In the case of a bubble of glycerine and ammonium oleate, the analysis for most purposes consisted simply in heating the liquid—in an oven at 90°–95° C.—until constant in weight. The contained glycerine and oleic acid were thus left practically in the same relative proportions as in the original bubble, water and ammonia being driven off. If necessary, the ammonia was separately estimated by Nesslerising. This is one advantage attending the use of ammonium oleate in such bubbles, as compared with the oleates of a non-volatile base like soda or potash.

The arrangements employed are shown in Fig. 11. The blowing-tube A, with its nozzle N to carry the bubble, is supported in an india-rubber cork fitting the neck of a 10 to 12-litre tabulated "aspirator" vessel. D is a bulbed vent tube, and is packed uniformly, but not tightly, with cotton-wool moistened by glycerine. This allows any variations of atmospheric pressure to be equalised without endangering the purity of the internal atmosphere. B is the graduated collection tube fitted in the lower tubulation; its nozzle, inside, is directly under the drops which accumulate on the bubble. Before opening the stop-cock on B, to draw off the accumulated liquid, the stopper in D is removed to allow the equalisation of barometric pressure, otherwise the small quantities of liquid could not be smoothly withdrawn; alternatively, D may remain open. Before commencing observations any excess liquid is removed from the bubble by drainage along the rod C. This is also done very effectively by a small bundle of very thin glass rods, about $\frac{1}{4}$ mm.

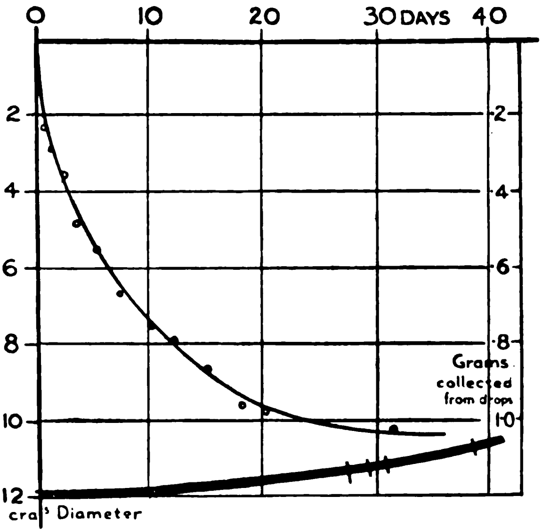

FIG. 12.—AIR BUBBLE OVER WATER REMAINS COLOURED.
Contraction and condensation.

diameter, tied together by thin aluminium wire; but the drawn-out glass rod C acts automatically when placed vertically just below the drop, for, as this grows, it descends by its own weight, and, enveloping the rounded end of the rod, drains down along the drawn-out neck, and the lightened bubble rises off the rod.

After the bubble has thus drained for several hours, the boiled distilled water is run in. The collecting tube may be utilised for this by turning down the nozzle and tilting the vessel, or a separate inlet tube with stop-cock may be fitted.

In order to isolate the bubble from the soap solution in A, and prevent any distillation or intrusion of extraneous liquid, the reservoir in some experiments was removable. For this purpose a conical ground joint was made in the tube A above the india-rubber cork.

The contraction of the bubble is somewhat irregular, because of the constantly varying thickness of the film, shown by the beautiful changes and gradings of colour. In the case of an air bubble the contraction is also relatively slow. Thus (Fig. 12) one of 12 cm. diameter only diminished in forty days to 11 cm. A second one, of 21·4 cm. diameter, contracted in five months to 16·6 cm. A third one, of 15·8 cm. diameter, took a little over six months to contract to a plane film on the supporting nozzle. In wet hydrogen, however (Fig. 13), an 11 cm. bubble,—under conditions of somewhat lower temperature,—contracted completely in about three months; almost all this time, moreover, it was of sufficient thickness to show strong colours.

The form of the contraction curve of the hydrogen bubble is fairly regular (considering the variations of thickness as shown by the colour changes), and of similar form to that given by black bubbles of constant composition. Its collection curve, of similar type, in which the amounts collected by drainage are plotted as ordinates to the time of collection as abscissae, is also shown in Fig. 13.

Similar contraction and collection curves are shown in Fig. 12 and Fig. 13 for air and hydrogen respectively. The left-hand vertical scales are for cms. in diameter for the contraction curves; the right-hand scales are for grams of liquid collected for the collection curves. The collection curve appears roughly as an image of the contraction curve, showing that, whereas the contraction is slow at first, increasing parabolically to the final disappearance, the condensation and drainage begins rapidly when the bubble contains concentrated glycerine, and falls off to zero as the dilution asymptotically approaches completion.

Careful measurement of such curves given by both air and hydrogen bubbles revealed them to be parabolic during the initial period before the surface became sensibly reduced by contraction. From this it follows that during this period the rate of collection is inversely as the total amount collected; just as in the contraction

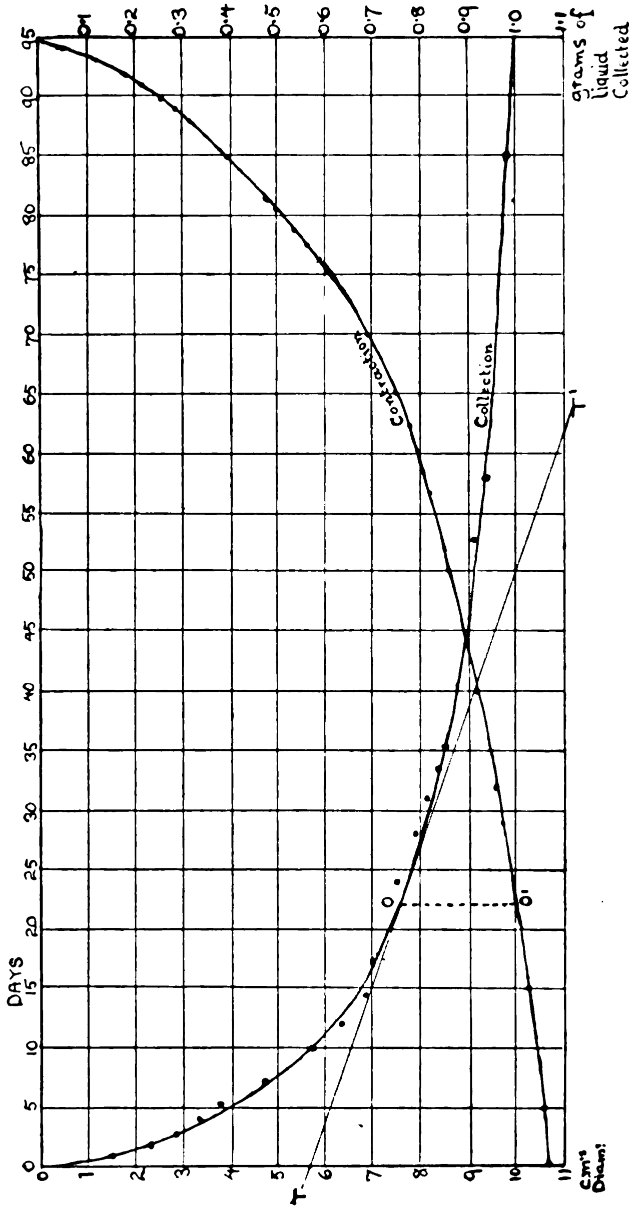

FIG. 13.—HYDROGEN BUBBLE IN HYDROGEN SATURATED WITH WATER VAPOUR. Contraction and water absorption.

curves the rate of diminution of diameter is inversely as the diameter. The equation of this first portion of the drainage curve shown in the above figure for a hydrogen bubble is $y = 0 \cdot 1787 \sqrt{x}$, where y is in grams, and x is in days.

The approach to agreement between the observed and calculated values thus resulting is seen in the following Table :—

TABLE 11.

x Days	. . . . .	1	2	3	4	6	8	10	12	14
y gms.	Observed .	0·18	0·26	0·305	0·36	0·45	0·52	0·57	0·62	0·66
	Calculated ·	0·18	0·25	0·309	0·36	0·44	0·51	0·57	0·62	0·67

In half a day 0·1 gram of liquid had been collected ; in eight days 0·5 gram was obtained ; but a total of 1·0 gram was only reached on the 95th day.

The corresponding expression in terms of mgms. from each sq. cm. of the bubble surface is $y = 0 \cdot 514 \sqrt{x}$. The result given by the bubble under similar conditions in air instead of hydrogen was $y = 0 \cdot 554 \sqrt{x}$. (See Fig. 12.)

The final composition of the bubble is thus relatively very little different from water. As a confirmation of this the original bubble solution (5 per cent. of ammonium oleate, in 50 per cent. glycerine) was diluted thirty-three times with carefully prepared boiled distilled water. A bubble 8 cm. diameter was then blown from this solution. It lasted until completely contracted after exactly four weeks. During most of this time it was coloured amber to purple ; on three or four of the warmer days nearer the end (temperature about 10° C.) it reached a semi-black stage peculiar to these very dilute bubbles. Only one drop accumulated, caused by the shrinking of the bubble. This bubble has already been referrred to in connection with gas transference through bubbles of graded dilution (see p. 24).

By investigating the condensation on unit area of the bubble, instead of on the whole bubble, a simple expression can be found that is applicable to the whole of the drainage curve, instead of only to the initial period of constant surface. The rate per unit area is of course found by dividing the weight of total drainage by the mean surface of the bubble over the period measured. This is most readily done by drawing a succession of tangents, TT′, to the collection curve (fig. 13). The slope of these successive tangents along the curve, gives the rates of collection at successive times from the whole bubble surface. The surface each time is obtained from the corresponding diameter, obtained by drawing the ordinates OO′ to cut the contraction curve. By dividing the total daily drainage rate given by the slope of the tangent TT′, by the corresponding surface at that time, there results the rate of condensation per unit area. The values thus accruing are set out in the first three horizontal rows of Table 12, with the corresponding day in the fourth row :—

TABLE 12.

Daily rate of condensation	0·049	0·031	0·0178	0·00885	0·0064	0·0047	0·0031	0·0015
Surface	353	346·3	333·3	314·2	283·5	254·5	227·0	153·9
Rate per unit area $\times 10^4$	13·9	0·895	0·534	0·282	0·226	0·185	0·137	0·097
Day	4	8	14	22	33	43	52	70

By plotting the third and fourth rows as ordinate and abscissa, a new and more fundamental curve is obtained, which is shown in Fig. 14.

An examination of this curve shows that the alteration of the rate of condensation *on unit area of the bubble* after the fourth day is practically hyperbolic as regards time. The expression most nearly fitting the curve is found (by plotting the results logarithmically) to be $x\,y = 0·705$, where x represents the time in days and y the milligrammes of condensation on each square centimetre. Calculated points are marked "x" on the diagram, and values obtained from observations as above are marked "⊙."

By bursting bubbles in a weighed vessel, it was found that at the mean thickness which these bubbles maintain under the given conditions (shown by their colour) they have a weight of very approximately 0·1 mgm. per square cm., and are therefore 0·001 mm. thick. Hence, in the equation just given, y may also represent the weight (in milligrammes) condensed per day on each 0·1 mgm. of the bubble. The resulting values calculated for certain days are set out in the following Table 13, together with the corresponding values obtained directly from the tangents to the original contraction curve :—

TABLE 13.

x = days		1	2	4	8	10	14	22	33	43	52	70
y = mgms. of condensation per mgm. of bubble . .	Calculated	0·705	0·352	0·176	0·090	3·071	0·050	0·032	0·021	0·016	0·014	0·010
	Observed .	—	—	0·130	0·088	—	0·053	0·028	0·023	0·019	0·014	0·010

The calculated values are at first too high, but later vary slightly on either side of those directly deduced from observations (see Fig. 14).

The values in milligrammes per day, of the rate of absorption on unit area with an air bubble 15½ cm. in diameter, are given in the subjoined Table 14. They were deduced from the weights of liquid collected in the periods named. The composition of the

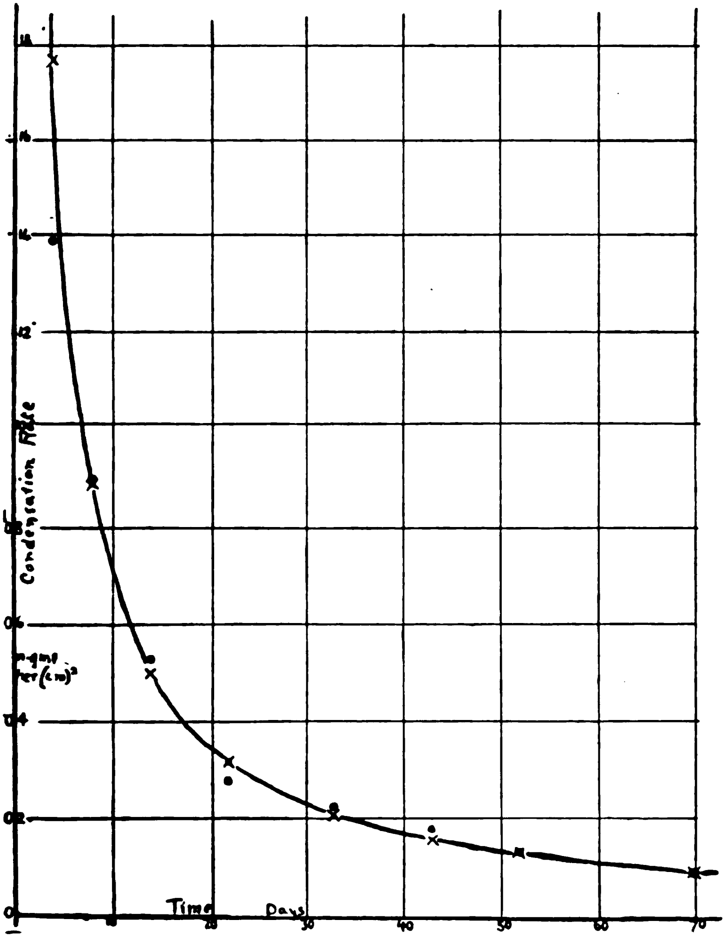

Fig. 14.—Condensation on Bubble in Wet Hydrogen.
Rate of condensation, plotted to time.

bubble was 50 per cent. of glycerine with 5 per cent. of ammonium oleate. This shows the rapid reduction in the daily rate of condensation occurring within a month,—namely, from 0·452 mgm. at the beginning down to 0·003 mgm. at the end.

TABLE 14.

Period	Ratio of Water-vapour Absorption
1st day	0·452
2-9 ,,	0·073
10-21 ,,	0·014
22-34 ,,	0·003

ALTERING COMPOSITION OF THE BUBBLE DRAINAGE.

The variation of the composition of the liquid draining from the bubble, during the early portion of these experiments, is shown in the next two diagrams.

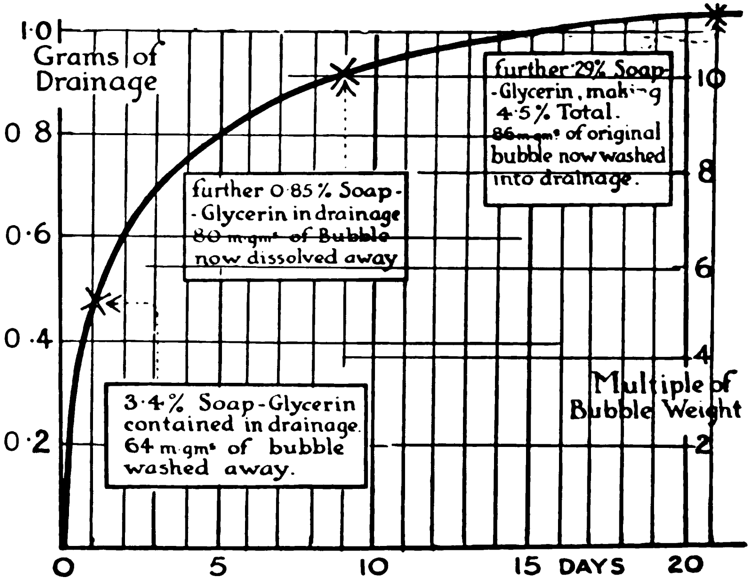

FIG. 15.—ALTERING COMPOSITION OF LIQUID COLLECTED FROM BUBBLE WHEN DILUTED BY CONDENSATION OF WATER. 16 cm. air bubble; 55 per cent. soap-glycerine.

Fig. 15 shows the parabolic increase in the weight of the "drainage," with time; on a separate scale of ordinates, on the right-hand side, is marked the multiple of the original bubble weight equal to the amount of liquid collected. The three points marked are:—

(1) 0·5 grm. of liquid in 1 day; being over 5 "bubble weights."
(2) 0·9 ,, ,, 9 days; ,, 10 ,, ,,
(3) 1·0 ,, ,, 21 ,, ,, 11 ,, ,,

At each of these points the composition of the drainage, as well as the actual weight of the bubble which had then been washed away, were as follows:—

(1) 3·4 per cent. soap-glycerine; 64 mgms. of original bubble washed away.

(2) Further 0·85 per cent. soap-glycerine; 80 mgms. of original bubble now washed away.

(3) Further 0·29 per cent. soap-glycerine; 86 mgms. of original bubble now washed away.

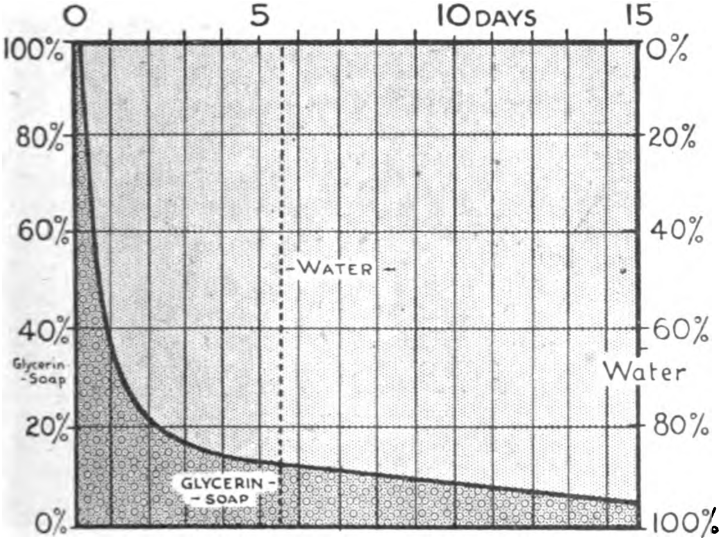

Fig. 16.—Altering Composition of Bubble when Diluted by Condensation of Water. 16 cm. air bubble.

In Fig. 16 the ordinates on the left show the decreasing percentage of soap-glycerine in the bubble, and on the right the consequent increase in the percentage of water, as the experiment proceeded. The number of days elapsing are again marked as abscissæ. The dotted ordinate shows, for example, that the bubble on the fifth day had nearly 90 per cent. of the original soap-glycerine replaced by water, and consequently only 10 per cent. of the soap-glycerine which was present in the original solution then remained; this was reduced on the fifteenth day to only about 3 per cent., but afterwards the rate of dilution was much less.

This intrinsic dilution curve agrees in form with that in Fig. 14, which shows the deduced rates of condensation per unit mass (0·1 mgm.) of the bubble with time. The one result follows from chemical measures of the alteration of drainage composition, while the other follows from the combined periodical observations of the weight of drainage and the contraction of the bubble.

The second part of this investigation dealing with Bubble-Complexes I hope to detail in a future Discourse.

W. J. Green, Esq., B.Sc., and J. W. Heath, Esq., F.C.S., Assistants of the Royal Institution, have aided in the course of the enquiry.

[J. D.]

LONDON: PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
GREAT WINDMILL STREET, W.1, AND DUKE STREET, STAMFORD STREET, S.E.1

WEEKLY EVENING MEETING,

Friday, January 25, 1918.

SIR WILLIAM PHIPSON BEALE, Bart., K.C. M.P.,
Vice-President, in the Chair.

PROFESSOR JOHN S. TOWNSEND, F.R.S.

The Motions of Ions in Gases.

WHEN ions move in a gas under an electric force, and the sizes of the ions are unaffected either by changes of force or of pressure, the velocity of the ions in the direction of the force is proportional to the ratio of the force to the pressure (X/P), provided the velocity in the direction of the force is small compared with the velocity of agitation of the ions. For small values of the ratio X/P the ions move as if they were associated with masses which are large compared with the masses of the molecules of the surrounding gas, and the kinetic energy of the motion of agitation of the ions is equal to that of the molecules with which they collide.

When the pressure is reduced the mobility of the negative ions increases with the force, which shows that the mass associated with each ion diminishes as the force increases.

Experiments were described from which it was shown that at a certain point, depending on the ratio X/P, the electrons move freely through the gas, as the ratio of the charge e to the mass m of the electrified particle was found to be approximately the same as the value $5 \cdot 6 \times 10^{17}$ for electrons moving in a highly exhausted space.

Thus in dry air at a pressure of 18 millimetres of mercury the electrons move freely, unassociated with larger masses, when the force is 3 or 4 volts per centimetre. A similar result is obtained at higher pressures by increasing the force. When a small quantity of water vapour is present in the gas a larger force is required to attain the point at which the electrons move freely, which shows that the electrons have a greater tendency to adhere to molecules of water vapour than to molecules of oxygen or nitrogen.

In all cases the electronic state is attained with much smaller forces than those required to produce ions by collisions with molecules. The latter effect, for example, could not be easily detected in air at 18 millimetres pressure unless the force exceeded 30 volts per centimetre.

The experiments show that while the mass associated with the electron undergoes these changes the charge remains constant.

It was also found that after the transition stage from the large to the small mass, the velocity of agitation of the electrons is much larger than the velocity corresponding to that of a particle of equal mass in thermal equilibrium with the surrounding gas. The latter velocity is approximately 10^7 centimetres per second, whereas the velocity of agitation of the electrons is $1 \cdot 6 \times 10^7$ centimetres per second in dry air, when $X/P = 0 \cdot 2$. This velocity increases with the force and is 10^8 centimetres per second, when $X/P = 50$.

The velocity of agitation of the electrons is easily deducted from observations on the lateral diffusion of a stream of particles moving through the gas under an electric force. The spreading of the stream is independent of the pressure of the gas, and has a definite normal value depending on the force when the kinetic energy of agitation of the ions or electrons is equal to that of the surrounding molecules. When the pressure is reduced below a certain value, which is proportional to the force, the lateral diffusion becomes abnormally large, which can only be due to an increase in the velocity of agitation of the electrons.

This result* shows that the electrons tend to retain the velocity they acquire under the electric force, and the principal effect of a collision with a molecule is not to reduce the velocity of an electron, but only to alter its direction of motion. This action continues until a steady state is attained in which the motion of agitation is so large that the loss of energy by collisions is equal to the energy acquired under the electric force.

[J. S. T.]

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* Proc. Roy. Soc., 1908, A, 81, p. 464.

WEEKLY EVENING MEETING,

Friday, February 1, 1918.

J. H. BALFOUR BROWNE, K.C. LL.D., Vice-President,
in the Chair.

PROFESSOR A. S. EDDINGTON, F.R.S.

Gravitation and the Principle of Relativity.

THERE were many difficulties to encounter in entering the room just now. To begin with, we had to bear the crushing load of the atmosphere, amounting to 14 lbs. on every square inch. At each step forwards, it was necessary to tread gingerly on a piece of ground moving at the rate of 20 miles a second on its way round the sun. We were poised precariously on a globe, apparently hanging by our feet, head outwards into space. And this acrobatic feat was performed in the face of a tremendous wind of ether, blowing at I do not know how many miles a second literally through us. We do not claim much credit for overcoming these difficulties—because we never noticed them. But I venture to remind you of them, because I am about to speak of some other extraordinary things that may be happening to us of which we are quite unconscious.

Not to go too far back in history, the present subject arises from a famous experiment performed in the year 1887, known as the Michelson-Morley experiment. The apparatus was elaborate, but the principle of the experiment is not very difficult. If you are in a river, which will be the quicker—to swim to a point 50 yards up stream and back again, or to a point 50 yards across stream and back again? Mathematically the answer is, perhaps, not immediately obvious, because the net effect of the current is a delay in both cases. But I think that anyone who has swum in a river will have no hesitation about the answer. The up-and-down journey takes longer. Now we are in a river—of ether. There is a swift current of ether flowing through this room; or, if we happen to be at rest in the ether at the present moment, six months hence the earth's orbital motion will be reversed, and then there must be a swift current. Michelson divided a beam of light into two parts; he sent one half swimming up the stream of ether for a certain distance, and then by a mirror back to the starting-point; he sent the other half an equal distance (as he thought) across the stream and back. It was a race; and with his apparatus he could test very accurately which part got back first. To his surprise, it was a dead-heat. Clearly the two paths could not really have been equal, the along-stream path must have been a little shorter to compensate for the greater hindrance of the current. That objection was foreseen, and the apparatus, which was mounted on a stone pier floating in mercury, was rotated through a right angle, so that the arm which was formerly along the stream was now across the stream, and *vice versa*. Again, the two portions of the beam arrived at the same moment; so this time the other arm had become the shorter—simply by altering its position. In fact these supposedly rigid arms had contracted when placed in the up-and-down stream position by just the amount necessary to conceal the effect which was looked for.

That is the plain meaning of the experiment; but we might well hesitate to accept this straightforward interpretation, and try to evade it in some way, were it not for some theoretical discoveries made later. It has gradually appeared that matter is of an electrical nature, and the forces of cohesion between the particles, which give a solid its rigidity, are electrical forces. Larmor and Lorentz discovered that this property of contraction in the direction of the ether current was something actually inherent in the formulæ for electrical forces written down by Maxwell many years earlier and universally adopted; it only waited for some mathematician to recognize it. It would be going too far to say that Maxwell's equations actually prove that contraction must take place; but they are, as it were, designed to fall in line with the contraction phenomenon, and certain details left vague by Maxwell have since been found to correspond.

We are then faced with the result that a material body experiences a contraction in the direction of its motion through the ether. According both to theory and experiment the contraction is the same for all kinds of matter—a universal property. One reservation should be made; the experiment has only been tried with solids of laboratory dimensions, which are held together by *cohesion*. There is at present no experimental evidence that a body such as the earth whose form is determined by *gravitation* will suffer the same contraction; we shall however assume that the contraction takes place in this case also.

I am going to ask you to suppose that we in this room are travelling through the ether at the rate of 161,000 miles a second, vertically upwards. Let us be bolder and say that that *is* our velocity through the ether—because no one will be able to contradict us. No experiment yet tried can detect or disprove that motion; because all such experiments give a null result, as the Michelson-Morley experiment did. With that speed the contraction is just one-half. This pointer, which I hold horizontally, is 8 feet long. Now [turning it vertically] it is 4 feet long. But, you may say, it is taller than I am, and I must be approaching 6 feet. No, if I lay down on the floor I should be, but as I am standing now I am under 3 feet. The contraction affects me just as it did the pointer. It is no use bringing a standard yard-measure to measure me, because that also will contract and represent only half a yard. "But we saw that the pointer did not change length when it turned." How did you tell that? What you perceived was an image of the pointer on the retina of your eye, and you thought the image occupied the same space of retina in both positions; but your retina has also contracted in the vertical direction without your knowing it, so that your estimates of length in that direction are double what they should be. And similarly with every test you could apply. If everything undergoes the same change, it is just as though there were no change at all.

We thus get a glimpse of what from our present point of view must be called the *real* world, strangely different from the world of appearance. In the real world, by changing position you extend yourself like a telescope; and the stoutest individual may regain slimness of figure by an appropriate orientation. It must be something like what we see in a distorting mirror; and you can almost see a living-picture of this real world reflected in a polished door-knob.

If our speed through the ether happens not to be so great as we have supposed, the contraction is smaller; but it escapes notice in our practical life, not because it is small, but because from its very nature it is undetectable. And because this real world is undetectable we do not as a rule attempt to describe it. Not merely in every-day life, but in scientific measurements also, we describe the world of appearance. We do this by imagining natural objects to be placed, not in the absolute space, but in a quite different framework of our own contriving—a space which corresponds to appearance. In the space of appearance a rod does not seem to change length when its direction is altered; and we use that property to block out our conventional space, counting the length occupied by the standard yard-measure as always a yard however its true length may vary. It is found also that in like manner our time is a special time of our own, different from the time we should adopt if our motion through the ether were nil. This is a perfectly right procedure; it introduces no scientific inexactness, and it is more in accordance with the ordinary meaning attached to space and time; the only thing to remember is that this space and time framework is something peculiar to us, defined by our motion, and it has not the metaphysical property of absoluteness, which we have often unconsciously attributed to it.

Now let us visit for a moment the star Arcturus, which is moving relatively to us with a velocity of over 200 miles per second. Consequently its motion through the ether is different from ours, and the contraction of objects on it will be different. It follows that our conventional space would not be suitable for Arcturus, because it was specially chosen to eliminate our own contraction effects. There is a different space and a different time proper to Arcturus. We must then imagine each star carrying its own appropriate space and time according to its motion through the ether The space and time of one star will not fit the experience of individuals on another star.

The exact relation between the appropriate space and time of one star and the space and time of another was first brought out clearly by Minkowski; it is a very remarkable one. We recognize three dimensions of space, which we may take as up-and-down, right-and-left, backwards-and-forwards. If we go over to Ireland we still have the same space, but Ireland's up-and-down no longer corresponds to ours. The directions are inclined; and what is vertical to them is partly vertical and partly horizontal to us. Now let us add a fourth dimension, imaginary* time, at right angles to the other three. There is no room for it in the model, but we must do our best to imagine it in four dimensions. In Ireland the three space-dimensions will have rotated, as I have said; but the time will be just the same. But if we go to Arcturus, or to any body moving with a velocity different from our own, the time-dimension also has rotated. What is time to them is partly time and partly imaginary space to us. It is a change in the space-time world of four dimensions just analogous to the change in the space-world between here and Ireland. That is Minkowski's great result; space-time is the same universally, but the orientation—the resolution into space and time separately—depends on the motion of the individual experiencing it, just as the resolution of space into horizontal and vertical depends on his situation. In Minkowski's own famous words—"Henceforth Space and Time in themselves vanish to shadows, and only a kind of union of the two preserves an independent existence."

From our original point of view it seems very remarkable that in the Michelson-Morley experiment the contraction should have been of just the right amount to annul the expected effect of our motion through the ether. Many other experiments, which seemed likely to show such an effect, have been tried since then, but in all of them the same kind of compensation takes place. It looks as though all the forces of nature had entered on a conspiracy together with the one design of preventing us from measuring or even detecting our motion through the ether. It is still an open question whether one force, the force of gravitation, has joined the conspiracy. Hitherto gravitation has stood aloof from all the other interrelated phenomena in majestic isolation. We have become almost reconciled to leaving it outside every physical theory. A new model of the atom is put forward which accounts for a whole host of abstruse and recently discovered properties; but it would be considered unfair to suggest that it ought to account for the simple and universal property of gravitation. Dare we think that gravitation has so far forgotten its dignity as to join this conspiracy? There is certainly not enough evidence for a jury to convict; but yet I think we shall have to intern it on suspicion. Recently Sir Oliver Lodge, believing that gravitation was innocent of the conspiracy, showed that a very famous astronomical discordance in the motion of Mercury might be an effect due to the sun's motion through the ether, and might afford a means of estimating its speed. It is difficult in a brief reference to deal quite fairly with an intricate question, but it seems now that we should rather lay stress, not on this single discordance, which can perhaps be otherwise explained, but on the exact agreement of Venus and the Earth with theory; for they also should show evidence of the sun's motion through the ether if gravitation had not joined in the conspiracy to conceal all such effects. It may be that the effects on Venus and the Earth are not found because the sun's motion through the ether happens to be very small; but on the whole it appears more likely that the effect of the motion is null, just as in the Michelson-Morley experiment, because there is a complete compensation in the law of gravitation itself.

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* Imaginary in the mathematical sense, i.e. involving $\sqrt{-1}$.

The great advantage of Minkowski's point of view is that it gets rid of all idea of a conspiracy. You cannot have a conspiracy of concealment when there is nothing to conceal. We cut Minkowski's space-time world in a certain direction, so as to give us separately space and time as they appear to us. We have been imagining that there exists some direction which would separate it into a real and absolute space and time. But why should there be? Why should one direction in this space-time world be more fundamental than any other? We do not attempt to cut the space-world in a particular direction so as to give us the *real* horizontal and vertical. The words horizontal and vertical have no meaning except in reference to a particular spot on the earth. So for a particular observer the space-time world falls apart into its four components, up-and-down, right-and-left, backwards-and-forwards, sooner-and-later; but no observer can say that this division is the one and only real one.

Our idea of a real space more fundamental than our own was, however, not entirely metaphysical; we had materialized it by filling it with an ether supposed to be at rest in it. We now deny the existence of any unique framework of that kind. We have failed to obtain experimental knowledge of such a framework since we cannot detect our motion relative to it. Whatever may be the nature of the ether, it is devoid of those material properties which could constitute it a framework of reference in space. We can perhaps best picture the ether as a four-dimensional fluid filling uniformly Minkowski's space-time *continuum*, not as a material three-dimensional fluid occupying space and time independently.

The position we have now reached is known as the Principle of Relativity. In so far as it is a physical theory, it seems to be amply confirmed by numerous experiments (except in regard to gravitation).

In so far as it is a philosophical theory, it is no more than a legitimate and useful point of view. I now pass on to a Generalized Principle of Relativity, in which we must be content at first to be guided by a natural generalization of these results, hoping later to be able to check our tentative conclusions by experiment.

If we analyze any scientific observation, distinguishing between what we perceive and what we merely infer, it always resolves itself into a *coincidence* in space and time. A physicist states that he has observed that the current through his coil is 5 milliamperes; but what he actually saw was that the image of a wire thrown by his galvanometer *coincided* with a certain division on a scale. He measures the temperature of a liquid, but the observation is the *coincidence* of the top of the mercury with a division on the thermometer. If then we had to sum up the whole of our experimental knowledge, we should have to describe it as consisting of a large number of coincidences.

A complete history of the progress of a particle consists of a knowledge of its path and the time at which it occupied each point of the path. The time may be regarded as an extra co-ordinate corresponding to a fourth dimension, and so the whole history may be summed up by a line in four dimensions representing the particle's progress through space and time. We call this four-dimensional line the *world-line* of the particle. Imagine that we have drawn the world-lines of all the particles, light-waves, etc., in the universe; we shall then have a complete history of the universe. It will be rather a dull history-book; the Venus of Milo will be represented by an elaborate schedule of measurements, and Monna Lisa by a mathematical specification of the distribution of paint; still they are there, if only we can recognize them. I have here a history of the universe —or part of it. Unfortunately I was not able to draw it in four dimensions, and even three dimensions presented difficulties, so I have drawn the world-lines in two dimensions on the surface of a football bladder.

A great deal is shown here which properly speaking is not history at all, because it is necessarily outside experience. As we have seen, it is only coincidences—the intersections of the world-lines—that constitute observational knowledge; and, moreover, it is not the place of intersection but the fact of intersection that we observe. I am afraid the two-dimensional model does not give a proper idea of this, because in two dimensions any two lines are almost bound to meet sooner or later; but in three dimensions, and still more in four dimensions, two lines can and usually do miss one another altogether, and the observation that they do meet is a genuine addition to knowledge.

When I squeeze the bladder the world-lines are bent about in different ways. But I have not altered the history of the universe, because no intersection is created or destroyed, and so no observable event is altered. The deformed bladder is just as true a history of nature as the undeformed bladder. The bladder represents Minkowski's space-time world, in which the world-lines were drawn; so we can squeeze Minkowski's world in any way without altering the course of events. We do not usually use the vulgar word squeeze; we call it a *mathematical transformation*, but it means the same thing.

The laws of nature in their most general form must describe correctly the behaviour of the world-lines in either the undistorted or the distorted model, because it is indifferent which we take as the true representation of the course of nature. That is a very important principle; but, being almost a truism, it does not in itself help us to determine the laws of nature without making some additional hypothesis. There is one law—the law of gravitation—which especially attracts our attention at this point, and we shall look into it more closely.

We know that one particle attracts another particle, and so influences the history of its motion. This evidently means that one world-line will deflect any other world-line in its neighbourhood. Apart from this influence, the world-line runs straight, bending neither to the right nor to the left, provided the bladder is in its undistorted state, i.e. provided we use Minkowski's original space-time. That is not so much a matter of observation as of definition. It defines what we are to regard as the undistorted state, though it is by observation that we learn that it is possible to find a space-time in which the world-lines run straight when undisturbed by gravitational or other forces. I must own that there is a certain logical difficulty in saying that a world-line runs straight when there are no others near it; because in that case there could be no intersections, and we could learn nothing about its course by observation. However, that is not a serious difficulty, though you may be reminded of the sage remark, "If there were no matter in the universe, the law of gravitation would fall to the ground."

We have to admit, then, that a world-line can be bent by the proximity of other world-lines. It can also be bent, as you see, by the proximity of my thumb. The suggestion arises, may not the two modes of bending be essentially the same? The bending by my thumb (a mathematical transformation of space and time) is in a sense spurious; the world-line is pursuing a course which is straight relative to the *original* material. Or we may perhaps best put it this way - the world-line still continues to take the shortest path between two points, only it reckons distance according to the length that would be occupied in the unstretched state of the bladder. It is suggested that the deflection of a world-line by gravitation is of the same nature; from each world-line a state of distortion radiates, as if from a badly puckered seam, and any other world-line takes the shortest course through this distorted region, which would immediately become straight if the strain could be undone. The same rule—of shortest distance as measured in the undistorted state—is to hold in all cases. This is a mode of reasoning which has often been fruitful in scientific generalizations. A magnetic needle turns towards the end of a bar-magnet; it also turns towards a spot near the pole of the earth; hence the suggestion that the earth is a magnet. We assume the essential identity of the two modes of deflecting the needle. It is a daring step to apply the analogy, and assume the essential identity of the two ways of deflecting world-lines: but at any rate we shall make this assumption and see what comes of it.

You will see that according to this view the earth moves in a curved orbit, not because the sun exerts any direct pull, but because the earth is trying to find the shortest way through a space and time which have been tangled up by an influence radiating from the sun. We can continue to describe this indirect influence of the sun on the earth's motion as a "force"; but, assuming that it makes itself felt as a modification or strain of space and time, we are able to bring the discussion of the laws of this force into line with the discussion of the laws of space and time, i.e. the laws of geometry. Needless to say we could not determine a physical law like the law of gravitation by geometrical reasoning without making some assumption.

I am afraid that to talk of a force as being a distortion of space and time must at first appear to you hopeless jargon. But it must be remembered first that we are not concerned with any metaphysical space and time. We mean by space and time simply a scaffolding that we construct as the result of our measures; and if anything queer happens to our measuring apparatus, the scaffolding may easily go crooked. Taking our everyday conception of space, we should say that this room is at rest; we have been told that it is being carried round the earth once a day, but in practical life we never pay any attention to that. The space that we naturally use is thus different from, and it is not difficult to show that it is distorted as compared with, the more fundamental astronomical space in which this room is travelling at a great velocity. So our scaffolding is crooked. But, it may be asked, in what way can this distortion of our space-scaffolding be regarded as a force? The answer is quite simple. We perceive it as a force, and that is the only way in which we do perceive it. We do not perceive that this room is being carried round by the earth's rotation, but we perceive a certain force—the earth's centrifugal force. It is rather difficult to demonstrate this force, because graviation predominates overwhelmingly; but if gravity were annihilated we should have to be tied down to the floor to prevent our flying up to the ceiling, and we should certainly feel ourselves pulled by a very vigorous centrifugal force. That is our only perception of the crookedness of our scaffolding.

We often call the centrifugal force an "unreal" force, meaning that it arises simply from a transformation of the framework of reference. Can we feel confident that gravitation is in any sense more "real"? In effect they are so much alike that even in scientific work we speak of them in one breath. What is called the value of gravity in London, 981·17 cm./sec², is really made up partly of the true attraction of the earth and partly of the centrifugal force. It is not considered worth while to make any distinction. Surely, then, it is not a great stretch of the imagination to regard gravitation as of the same nature as centrifugal force, being merely our perception of the crookedness of the scaffolding that we have chosen.

If gravity and centrifugal force are manifestations of the same underlying condition it must be possible to reduce them to the same laws; but we must express the laws in a manner which will render them comparable. There is a convenient form of Newton's law, which was given by Laplace and is well known to mathematicians, which describes how the intensity at any point is related to the intensity at surrounding points—or, according to our interpretation, how the distortion of space at any point fits on to the distortion at surrounding points. It is evidently an attempt to express the general laws of the strains in space and time which occur in nature. If we are correct in our assumption that gravitation involves *nothing more* than strain of space-time,* so that its law expresses merely the relation between adjacent strains which holds by some natural necessity, clearly the strains which give the centrifugal force must obey the same general law. Here a very interesting point arises. We cannot reconcile the Newtonian law of gravitation with this condition. Newton's law and the law of centrifugal force are contradictory.

To put the matter another way, if we determine the strains by Newton's law, we get results closely agreeing with observation, provided Minkowski's space-time is used; but if we avail ourselves of our right to use a transformed space-time, the results no longer agree with observation. That means that Newton's law involves something which is not fully represented by strains, and so does not agree with our assumption. We must either abandon our assumption, or abandon the famous law which has been accepted for over 200 years, and find a new law of gravitation which will fall in with our requirements.

This amended law has been found by Einstein. It appears to be the only possible law that meets our requirements, and in the limited applications which come under practical observation is sufficiently close to the old law that has served so well. In practical applications the two laws are indistinguishable, except for one or two crucial phenomena to which reference will be made later. But in gravitational fields far stronger than any of which we have experience, and for bodies moving with velocities much greater than those of the planets, the difference would be considerable.

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* The idea is that matter represents a seam or nucleus of strain, and the strains at other points link themselves on according to laws inherent in the *continuum* and quite independent of the matter. The matter starts the strain, but does not *control* it as it goes outwards.

This idea of the distortion of space as the *modus operandi* of gravitation has led to a practical result—a new law of gravitation. It is not brought in as a hypothetical explanation of gravitation; if Einstein's theory is true, it is simply of the nature of an experimental fact.

If we draw a circle on a sheet of paper and measure the ratio of the circumference to the diameter, the result gives, if the experiment is performed accurately enough, the well-known number π, which has been calculated to 707 places of decimals. Now place a heavy particle at or near the centre and repeat the experiment; the ratio will be not exactly equal to π, but a little more. The experiment has not been performed, and is not likely to be performed, because the difference to be looked for is so small; but, if Einstein's theory is correct, that must be the result. The space around the heavy particle does not obey ordinary geometry; it is non-Euclidean. The change in its properties is not metaphysical, but something which with sufficient care could be measured. You can keep to Euclidean space if you like, and say that the measuring-rod has contracted or expanded according as it is placed radially or transversely to the gravitational force. That is all very well if the effect is small, but in a very intense gravitational field it would lead to ridiculous results like those we noticed in connection with the Michelson-Morley experiment—everything expanding or contracting as it changed position, and no one aware of any change going on. I think we have learnt our lesson that it is better to be content with the space of experience, whether it turns out to be Euclidean or not, and to leave to the mathematician the transformation of the phenomena into a space with more ideal properties.

This consequence of the new law of gravitation, though theoretically observable, is not likely to be put to any practical test either now or in the immediate future. But there are other consequences which just come within the range of refined observation, and so give an immediate practical importance to the new theory, which has indeed scored one very striking success. If we could isolate the sun and a single planet, then under the Newtonian law of gravitation the planet would revolve in an ellipse, repeating the same orbit indefinitely. Under the new law this is not quite true; the orbit is nearly an ellipse, but it does not exactly close up, and in the next revolution the planet describes a new ellipse in a slightly advanced position. In other words, the elliptic orbit slowly turns round in the same direction in which the planet is moving, so that after the lapse of many centuries the orbit will point in a different direction. The rate at which the orbit turns depends on the speed of motion of the planet in its orbit, so we naturally turn to the fastest moving planets, Mercury, Venus and the Earth, to see if the effect can be detected. Mercury moves at 30 miles a second; Venus at 22; the Earth at 18½. But there is a difficulty about Venus and the Earth. Their orbits are nearly circular, and you cannot tell in which direction a circle is pointing. Mercury combines the favourable conditions of a high speed and a satisfactorily elongated orbit whose direction at any time can be measured with considerable precision. It is found by observation that the orbit of Mercury is advancing at the rate of 574 seconds of arc a century. This is in great measure due to the attraction of the other planets, which are pulling the orbit out of shape and changing its position. The amount of this influence can be calculated very accurately, and amounts to 532 seconds per century. There is thus a difference of 42 seconds a century unaccounted for; and this has for long been known as one of the most celebrated discordances between observation and gravitational theory in astronomy. It is thirty times greater than the probable error which we should expect from uncertainties in the observations and theory. There are other puzzling discordances, especially in connection with the motion of the moon; but the conditions in that case are more complicated, and I scarcely think they offer so direct a challenge to gravitational theory. Now Einstein's theory predicts that there will be a rotation of the orbit of Mercury additional to that produced by the action of the planets; and it predicts the exact amount—namely, that in one revolution of the planet the orbit will advance by a fraction of a revolution equal to three times the square of the ratio of the velocity of the planet to the velocity of light. We can work that out, and we find that the advance should be 43 seconds a century—just about the amount required. Thus whilst the Newtonian law leaves a discordance of over 40 seconds, Einstein's law agrees with observation to within a second or so.

Of course this superiority would be discounted if we could find some other application where the old Newtonian law had proved the better. But that has not happened. In all other cases the two laws agree so nearly that it has not been possible to discriminate between them by observation. The new law corrects the old where the old failed, and refrains from spoiling any agreement that already exists. The next best chance of applying the new theory is in the advance of the orbit of Mars; here Einstein's new law "gilds refined gold" by slightly improving an agreement which was already sufficiently good —a "wasteful and ridiculous excess" which is at any rate not unfavourable to the new theory.

There is another possibility of testing Einstein's theory, which it is hoped to carry out at the first opportunity. This relates to the action of gravitation on a ray of light. It is now known that electromagnetic energy possesses the property of inertia or mass, and probably the whole of the mass of ordinary matter is due to the electromagnetic energy which it contains. Light is a form of electromagnetic energy, and therefore must have mass—a conclusion which has been found true experimentally, because light falling on any object exerts a pressure just as a jet of water would. We ordinarily measure mass in pounds, and it is quite proper to speak of "a pound of light," just as we speak of a pound of tobacco. In case anyone should be thinking of going to an Electric Light Company to buy a pound of light, I had better warn you that it is a rather expensive commodity. They usually prefer to sell it by a mysterious measure of their own, called the Board of Trade Unit, and charge, at least, threepence a unit. At that rate I calculate that they would let you have a pound of light for £141,615,000. Fortunately, we get most of our light free of charge, and the sun showers down on the earth 160 tons daily. It is just as well we are not asked to pay for it.

But although light has mass, it does not follow that light has weight. Ordinarily, mass and weight are associated in a constant proportion, but whether this is so in the case of light can only be settled by experiment—by weighing light. It seems that it should be just possible to do this. If a beam of light passes an object which exerts a gravitational attraction, then, if it really has weight, it must drop a little towards the object Its path will be bent just as the trajectory of a rifle bullet is curved owing to the weight of the bullet. The velocity of light is so great that there is only one body in the solar system powerful enough to make an appreciable bend in its path, namely, the sun. If we could see a star close up to the edge of the sun, a ray of light coming from the star would bend under its own weight, and the star would be seen slightly displaced from its true position. During a total eclipse stars have occasionally been photographed fairly close to the sun, and with care it should be possible to observe this effect. There is a magnificent opportunity next year when a total eclipse of the sun takes place right in the midst of a field of bright stars. This is the best opportunity for some generations, and it is hoped to send out expeditions to the line of totality to weigh light according to this method.

In any case great interest must attach to an attempt to settle whether or not light has weight. But there is an additional importance, because it can be made a means of confirming or disproving Einstein's theory. On Einstein's theory light must certainly have weight, because mass and weight are viewed by it as two aspects of the same thing; but his theory predicts a deflection twice as great as we should otherwise expect. Apart from surprises, there seem to be three possible results: (1) a deflection amounting to 1·75″ at the limb of the sun, which would confirm Einstein's theory; (2) a deflection of 0·83″ at the limb of the sun, which would overthrow Einstein's theory, but would establish that light was subject to gravity; (3) no deflection, which would show that light though possessing mass has no weight, and hence that Newton's law of proportionality between mass and gravitation has broken down in another unexpected direction.

The purpose of Einstein's new theory has often been misunderstood, and it has been criticized as an attempt to explain gravitation. The theory does *not* offer any explanation of gravitation; that lies quite outside its scope, and it does not even hint at a possible mechanism. It is true that we have introduced a definite hypothesis as to the relation between gravitation and a distortion of space; but if that explains anything, it explains not gravitation but space, i.e. the scaffolding constructed from our measures. Perhaps the position reached may be made clearer by another analogy. Let us picture the particle which describes a world-line as hurdle-racer in a field thickly strewn with hurdles. The particle in passing from point to point always takes the path of least effort, crossing the fewest possible hurdles; if the hurdles are uniformly distributed, corresponding to undistorted Minkowskian space, this will, of course, be a straight line. If the field is now distorted by a mathematical transformation such as an earthquake so that the hurdles become packed in some parts and spread out in others, the path of least effort will no longer be a straight line; but it is not difficult to see that it passes over precisely the same hurdles as before, only in their new positions. The gravitational field due to a particle corresponds to a more fundamental rearrangement of the hurdles, as though someone had taken them up and replanted them according to a law which expresses the law of gravitation. Any other particle passing through this part of the field follows the guiding rule of least effort, and curves its path if necessary so as to jump the fewest hurdles. Now, we have usually been under the impression that when we measured distances by physical experiments we were surveying the *field*, and the results could be plotted on a map; but it is now realized that we cannot do that. The field itself has nothing to do with our measurements; all we do is to count hurdles. If the only cause of irregularity of the hurdles were earthquakes (mathematical transformations) that would not make much difference, because we could still plot our counts of hurdles consistently as distances on a map; and the map would represent the original condition of the field with the hurdles uniformly spaced. But the more far-reaching rearrangement of hurdles by the gravitational field forces us to recognize that we are dealing with counts of hurdles and not with distances; because if we plot our measures on a map they will not close up. The number of hurdles in the circumference of a circle* will not be π times the number in the diameter; and when we try to draw on a map a circle whose circumference is more than π times its diameter, we get into difficulties—at least in Euclidean space. This analogy brings out the point that the theory is an explanation of the real nature of our measures rather than of gravitation. We offer no explanation why the particle always takes the path of least effort—perhaps, if we may judge by our own feelings, that is so natural as to require no explanation. More seriously, we know that in consequence of the undulatory theory of light, a ray traversing a heterogeneous medium always takes the path of least time; and one can scarcely resist a vague impression that the course of a material particle may be the ray of an undulation in five dimensions. What concerns gravitation more especially is that we have offered no explanation of the linkages by which the hurdles rearrange themselves on a definite plan when disturbed by the presence of a gravitating particle; that is a point on which a mechanical theory of gravitation ought to throw light.

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* A circle would naturally be defined as a curve such that the number of hurdles (counted along the path of least effort) between any point on it and a fixed point called the centre is constant. To make the vague analogy more definite, we may suppose that the hurdles are pivoted, and swing round automatically to face the jumper; he is not allowed to dodge them, i.e. to introduce into his path sinuosities comparable with the lengths of the hurdles.

From the constant of gravitation, together with the other fundamental constants of nature—the velocity of light and the quantum of action—it is possible to form a new fundamental unit of length. This unit is 7×10^{-28} centimetres. It seems to be inevitable that this length must play some fundamental part in any complete interpretation of gravitation. (For example, in Osborne Reynold's theory of matter this length appears as the mean free-path of the granules of his medium.) In recent years great progress has been made in knowledge of the excessively minute; but until we can appreciate details of structure down to the quadrillionth or quintillionth of a centimetre, the most sublime of all the forces of nature remains outside the purview of the theories of physics.

[A. S. E.]

APPENDIX.

OUTLINE OF THE MATHEMATICAL THEORY OF EINSTEIN'S LAW OF GRAVITATION.

The fundamental formula, by which from measurements we infer the relative positions of objects in a space defined by three rectangular co-ordinates, x, y, z, is—

$$ds^2 = dx^2 + dy^2 + dz^2 \qquad (1)$$

where ds is the *measured* element of length, and the right-hand side refers to the *inferred* positions. Experiments are concerned with fields of gravitation which from the present point of view must be regarded as extremely weak, so the formula must be taken as applying strictly only in the absence of gravitation. (We have no proof that in a strong gravitational field the formula would be self-consistent, i.e. that measured space would be Euclidean.)

In four dimensions, the formula is generalized to—

$$ds^2 = dx^2 + dy^2 + dz^2 - dt^2 \qquad (2)^*$$

Here, again, ds is a measured quantity (partly by scales and partly by clocks), and the right-hand side refers to the inferred locations. The units are chosen so that the velocity of light is unity. According to the old theory of relativity the measured "distance," ds, between two events is not affected by any uniform motion of the observer.

If four new co-ordinates x_1, x_2, x_3, x_4, which are arbitrary functions of x, y, z and t, are introduced and substituted in the right-hand side of (2), we obtain an equation of the general form—

$$ds^2 = g_{11}\,dx_1^2 + g_{22}\,dx_2^2 + \ldots + 2g_{12}\,dx_1\,dx_2 + 2g_{13}\,dx_1\,dx_3 + \ldots \qquad (3)$$

where the g's are functions of the co-ordinates. Cubes and higher powers of the infinitesimals can be neglected.

In natural rectangular co-ordinates the path of a particle under no forces is a straight line described with uniform velocity, or more briefly a straight line in four dimensions. This may be expressed in a form which is independent of the choice of co-ordinates, viz. $\int ds$ is a minimum. By substituting under the integral the value of ds from (3) and applying the calculus of variations, we obtain the general equations of motion under no forces applicable to any system of co-ordinates. The g's and their derivatives will occur in these equations.

In particular, by taking x_1, x_2, x_3, x_4 to be rectangular axes rotating with the earth, we should obtain the equations of motion of a particle under no "real" forces referred to those axes—in other words, the equations of motion of a particle in a field of centrifugal force. The centrifugal force enters into the equations through the intermediary of the corresponding g's; and we thus get the notion of a field of force as defined by a set of values of the g's. Our hypothesis of the complete equivalence of gravitation to forces like the centrifugal force arising from a transformation of the axes of reference shows that we may also define the gravitational field by a set of values of the g's. In the case of the centrifugal force the values of the g's are such that by a transformation of the co-ordinates we can transform (3) to (2). It does not necessarily follow that this can be done when the g's have values corresponding to a gravitational field; and in fact we cannot do it for a *finite* region of space, although, of course, in an infinitesimal element gravitation may be made to disappear by an appropriate transformation.

The g's defining the gravitational field may be regarded as the ten components of a generalized gravitational potential. In fact, in rectangular co-ordinates one of them, g_{44}, corresponds to twice the Newtonian potential.† Newton's law is therefore expressed by Laplace's equation—

$$\nabla^2 g_{44} = 0 \qquad (4)$$

in free space. It is impossible to accept this as a general law satisfied by the g's, because, for example, it is not satisfied when the g's represent a centrifugal force. Clearly the hypothesis of equivalence requires that there should be one or more general differential equations satisfied by the g's in all cases, and not a special law satisfied by gravitational g's and another satisfied by distortion g's. If, then, the law is a general relation between the g's, it must hold for all systems of co-ordinates; that is, it must be co-variant for all transformations.

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* This formula is usually given with the reversed sign.

† It is an easy illustration to work out the transformation of (2) to rotating axes, when it will be found that g_{44} is twice the potential of the corresponding centrifugal force.

The general condition satisfied by the g's *in the absence of a gravitational field* is written in the form—

$$B^{\rho}_{\mu\sigma\tau} = 0 \qquad (5)$$

where the quantity on the left is known as the Riemann-Christoffel tensor. (The word *tensor* expresses the property, that if it vanishes in one system of co-ordinates it vanishes in all systems.) It is a function of the g's and their first and second derivatives with respect to x_1, x_2, x_3, x_4. It has 256 components, formed by ringing the changes on the suffixes ρ, μ, σ, τ, giving them the values 1, 2, 3, 4.* By symmetry many of these are identical, and we actually get (I think) 96 apparently different equations, some of which may not be independent. The equation (5) is to be understood to mean that all the 96 components vanish. The equation expresses the fact that a mathematical transformation exists which can transform (3) to (2) throughout space; and that is the method by which it is obtained analytically.

The general equation between the g's, allowing for a gravitational field, must be less stringent; it must be such that it is satisfied when (5) is satisfied, but not necessarily *vice versa*. (Zero gravitation is a particular case of gravitation, but not *vice versa*.) The simplest symmetrical law that we could propose is—

$$B_{\mu\sigma} = \sum_{\rho=1}^{4} B^{\rho}_{\mu\sigma\rho} = 0 \qquad (6)$$

This is clearly satisfied when (5) is satisfied.

$B_{\mu\sigma}$ is called the reduced (*verjüngt*) Riemann-Christoffel tensor, and has ten different (but not all independent) components. It seems to be the only possible way of symmetrically building up another tensor out of the components of $B^{\rho}_{\mu\sigma\tau}$; and it appears also that equation (6) is the only co-variant equation of the second rank (i.e., having ten components) that can be formed from the g's, and their first and second derivatives and linear in the last. Co-variant equations of higher rank (with more components) would impose too great restrictions, and like the Riemann-Christoffel tensor would not admit a gravitational field.

For this reason (6) is chosen as the new law of gravitation. It reduces to the Newtonian law as first approximation.

It remains to see how the equation (6) is modified when the space is occupied by mass, i.e. electromagnetic energy. What is to be the new form of Poisson's equation $\nabla^2\phi = -4\pi\rho$? It is found that equation (6) can be transformed into a Hamiltonian form—

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* It would be cumbrous to write down the value of $B^{\rho}_{\mu\sigma\tau}$; but it will be understood that it contains $g_{\mu\sigma}$, x_{ρ}, etc., and the different components are got by giving the values 1, 2, 3, 4 to the suffixes.

$$\delta(\int \mathrm{H}\, d\tau) = 0 \tag{7}$$

where $d\tau$ is a four-dimensional element of volume, and H a certain function of the g's and their derivates.

The electromagnetic equations of Maxwell in the absence of a gravitational field can also be expressed in a Hamiltonian form—

$$\delta(\int \mathrm{H}'\, d\tau) = 0 \tag{8}$$

where H′ is a function of the quantities defining the electromagnetic field.

It is clear that we must form the general equations, when gravitation and electromagnetic forces are both present, by combining (7) and (8) thus—

$$\delta(\int(\mathrm{H} + \lambda \mathrm{H}')\, d\tau) = 0 \tag{9}$$

The constant λ, whose value cannot be predicted *a priori*, indicates the relation between the gravitational and electromagnetic effects caused by the same mass, and corresponds to the constant of gravitation.

The mathematical operations, omitted in this brief sketch, are long and rather difficult; but it is hoped that it may enable the reader to gather the general nature of the argument.

GENERAL MONTHLY MEETING,

Monday, February 4, 1918.

Sir James Crichton-Browne, Treasurer, in the Chair.

J. Turner MacGregor-Morris,
Professor Paule Popoire,

were elected Members.

The Managers reported: That Dr. Mond, under the Conveyance and Deed of Trust of the Davy Faraday Research Laboratory of the Royal Institution, covenanted to pay to the Royal Institution before the year 1926, the sum of £62,000 as Endowment Fund. The Trustees have in the most generous way anticipated the obligation by eight years, and have transferred the sum of £66,500 in 5% War Stock to the Trustees (nominated by the Managers) of the Davy Faraday Research Laboratory Endowment Fund. This will add materially to the Income available for the purpose of promoting and maintaining the efficiency of the Davy Faraday Research Laboratory, in the Advancement of Original Research in Chemical and Physical Science.

The Secretary announced the decease of Sir John Wolfe Barry, on January 22, 1918.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and the World of Science by the decease of Sir John Wolfe Barry, K.C.B., LL.D., F.R.S., Past President of the Institution of Civil Engineers, M.I.Mech.E., M.I.E.E.

Sir John Wolfe Barry was a Member of the Royal Institution for thirty-two years, and discharged the official duties of a Manager. He took an active interest in the welfare of the Institution, and as a Manager rendered invaluable service to the Institution. He was celebrated for many notable achievements in Engineering, and served on a number of Royal Commissions. He was also Joint Author of "Railways and Locomotives," in addition to other books.

Resolved, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Lady Barry and the Family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, February 8, 1918.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

PRINCIPAL E. H. GRIFFITHS, Sc.D. F.R.S.

Science and Ethics.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, February 15, 1918.

THE RIGHT HON. LORD RAYLEIGH, O.M. D.C.L. LL.D. F.R.S., in the Chair.

PROFESSOR W. BATESON, D.Sc. F.R.S.

Gamete and Zygote.

[ABSTRACT].

IN modern biology the ideas denoted by the terms Gamete and Zygote are fundamental. No one having fully appreciated the meaning of these ideas can look on the living world with quite the same eyes as before. The two expressions, Gamete and Zygote, were introduced, in the haphazard fashion too commonly followed by scientific men, to denote on the one hand the single, unpaired germs or germ-cells, and on the other the double or paired bodies which result from the union of two germ-cells in fertilization. The bodies of most animals and plants are *zygotes*, being the product of two *gametes* or "marrying" cells "yoked" together in a common system, the body. Except for differences in mobility and in the amount of food materials which they contain, there is so far as we know no general or essential distinction between the germ-cells contributed by the male and the female respectively. In the union of two such cells, the powers, bodily and mental, of the resulting organism are conveyed to it. By many competent observers it is believed that all these powers are conferred solely by the nuclear materials of the germ-cells; and from certain elaborate and valuable experiments Morgan and his associates have convinced themselves that the properties of organisms are determined by particles of nuclear material arranged in a predicable linear order. Nevertheless alternative hypotheses have not hitherto been adequately investigated. The evidence most favourable to the chromosome scheme of heredity is that furnished by the phenomenon called "non-disjunction" by Bridges. Here unusual zygotic types were consistently produced in association with the visible presence of definite aberrations in the chromosome numbers. Striking as this evidence is, it must still be possible to doubt whether the relations are certainly those of cause and effect.

The different qualities being introduced on fertilization, the problem of heredity is to discover the method and system by which these differences are segregated and distributed among the gametes produced by the zygote. If the gametes uniting in fertilization are identical in composition, those subsequently formed by the zygote will be like them, save in the rare and exceptional case of new and original variation. The divisions by which such cells are formed are *divisions of equality* or repetition. If the combining gametes were unlike, sooner or later in the germ-cell lineage *divisions of diversity* or differentiation must occur. Similarly in zygotic cells these two kinds of division are familiar, and thus an imperfect but profitable comparison may be instituted between the differentiating divisions by which the bodily organs are marked off, and those by which the dissimilar properties of the series of gametes are distributed. All permanence and all diversity are ultimately traceable to the process of cell-division.

The nuclei contributed by the father and by the mother commonly unite intimately, but in *Cyclops* they lie in pairs, side by side, in each cell throughout life. We have as yet no proof that recombinations of parental characters can occur in such a form as *Cyclops*, but on analogy we can scarcely doubt that they may. Experiments with such types might lead to discoveries as to nuclear functions.

The sterility of hybrids is a consequence of the failure to sort out or distribute the two sets of parental characters among the gametes.

As to the physical nature of segregation there is still no light whatever. Before we can form a conjecture regarding that process some correct knowledge as to mechanism of cell-division in general must be obtained. The geometrical appearances and the phenomena of polarity are the only available guides. No plausible representation of the physical process by which a cell divides into two equal halves has been devised, but in view of the experiments of Loeb and others showing that complete development may, in the case of the frog's egg, be excited by the prick of a needle (as well as by other stimuli), the conclusion is probable that fertilization acts by releasing a strain. Divisions of equality may occur such that from the division of the fertilized ovum two, four, six or more identical organisms may be formed (twins; quadruplets occurring normally in Armadillo, etc.). More curious are the cases in which a *group* of meristematic cells has the power of separating, developing axes, and becoming an embryo (*Crisia*, Harmer). We are here reminded of the observations of H. V. Wilson (confirmed by J. Huxley) that if certain sponges are pulped through a fine cloth, the separated cells can join together and sort themselves into fresh orderly aggregations. Compare also the fact that in Winkler's famous graft-hybrids in *Solanum*, a peripheral tissue derived from one species can fit itself to a core derived from another. Such facts plainly indicate that powers of orderly adjustment are possessed by living things, but the arguments of Driesch, that such powers are transcendental, seem altogether premature, and may be dismissed as counsels of despair.

The generalization may be hazarded that in somatic repetitions geometrical relations to the axes of the body are commonly maintained, whereas in gametic repetitions they are commonly lost. (Observe that in Barley, though the embryos stand in definite rows, back to back, each embryo may be either a right-handed or a left-handed screw.)

The phenomena of heredity are obviously related to those of symmetry and polarity exhibited in the repetitions of parts so characteristic of zygotic structures. Remarkable, and doubtless significant, distinctions exist between animals and plants in these respects. Plants which are mosaics or patchworks, presenting a mixture of allelomorphic characters *not subordinated to geometrical control*, are usually incapable of being bred true. The geometrical disorder is an indication that the distribution is a mere fortuitous collocation of dissimilar elements, and not a genetically transmissible pattern. Some of the gametes in such a plant will carry one or other of the components; others arising themselves as mosaic cells may repeat the mixture (cf. mosaic Azaleas, Carnations, the Bizarria Orange, etc.). Animals, however, having mosaic patterns may often be readily established as pure breeds (Sheeted Cows, Dalmatian Dogs, White Bantams, having one or more small grey ticks, etc.). The geometrical relations of gametes to zygotes are therefore quite distinct in animals and plants.

The geometrical phenomena of gametes are so far quite insufficiently studied. In some animals the spermatozoa are all right-handed, in others they are all left-handed; very rarely there is a mixture (Rat).

Remarkable phenomena of polarity are exhibited by the "giant forms" best known in certain plants. Just as a zygote can divide to form a twin-pair, so can the material which normally is distributed as two plants be compounded as a single "individual." The case most studied is that of *Primula sinensis*, described by R. P. Gregory. On at least two occasions "giant" plants containing double the usual number of chromosomes have arisen. Both of these sets of giants exhibit the unique property of being totally unable to breed with the plants from which they were derived, though fertile with other giants. Similar cases are known in Banana (Tischler). Species of Chrysanthemum are described as having respectively 9, 18, 27, 36, 45 chromosomes. Geoffrey Smith and others have observed giant sperms in animals which presumably contain extra chromosome material.

Marchal succeeded, by an ingenious method, in raising giant mosses containing double the material proper to a normal plant.

By virtue of its powers of grouping material around definite axes the zygote can be formed of less than the normal amount of material, or of more than the normal amount; and gametes can at all events exemplify this latter power, inasmuch as they may be compounded of multiples of the normal amount.

There are examples of *quantitative* changes in zygotes and gametes by which unions or divisions of equal and similar materials are effected. Analogy may be instituted between such unions and divisions and those produced by physical means in various forms of wave-motion.* The pattern, for instance, on Grévy's Zebra may be said, almost without extravagance, to be the upper octave of that on the Mountain Zebra.

Divisions of differentiation or segregation among gametes commonly distribute elements received as already dissimilar in fertilization. In *original variation*, by some irregularity in the process of division, dissimilarity must be introduced. Lotsy has doubted whether real proof of such contemporary variation exists, but cases apparently incapable of any other interpretation can be adduced. A profusion of new varieties has in modern times been produced, for instance, in both *Primula sinensis* and the Sweet Pea, though all attempts to cross these plants have failed. In such examples, moreover, there is clear evidence of new gradational forms arising as rarities after much intercrossing of varieties. The appearance of a continuous variation leading to extremes is illusory. Jennings, in the comparable instance of *Drosophila*, has argued that since almost every gradation from a pale red eye to a white eye now exists, the process of variation should be regarded as continuous. But in *Drosophila*, as in the Sweet Pea, *the extremes came first*, and intermediates have arisen by a process of fractionation, representing almost certainly irregularities in the perfection with which segregation has been accomplished. Such irregularities may be compared with the various degrees of impurity with which liquids incapable of mixing may be decanted from each other.

On the other hand, it is true that no satisfactory evidence of any new positive ingredient being introduced, whether into zygote or gamete, is yet before us.

[W. B.]

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* Cf. W. Bateson, "The Problems of Genetics," 1913, chap. iii.

WEEKLY EVENING MEETING,

Friday, February 22, 1918.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

ARTHUR CLUTTON-BROCK, M.A.

The Importance of Art.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, March 1, 1918.

SIR JAMES REID, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

PROFESSOR ARTHUR G. GREEN, M.Sc. F.R.S.

The Modern Dye-Stuff Industry.

[NO ABSTRACT.]

GENERAL MONTHLY MEETING,

Monday, March 4, 1918.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

G. L. Addenbroke, M.I.E.E.
Lady Audrey Crichton-Browne.
Thomas Barter Gilley,
Siward Myles Horsley,
Colonel Charles Robert Kerr,
Miss Rachel M. Parsons,
Arthur Ronald Trist,
Cornelius Verheyden,
Evan Williams, J.P.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Journal, Vol. XIII. Jan. 1918; Vol. XII. 1917.
India Association: Proceedings, Vols. III. & IV.
Department of Agriculture for India: Memoirs, Oct. 1917.
American Chemical Society—Journal of Industrial and Engineering Chemistry for Jan. 1918. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXVIII. No. 2. 8vo. Dec. 1917.
Bankers, Institute of—Journal, Jan. 1918.
Boston Public Library—Bulletin, Third Series, Vol. X. No. 4. 8vo. 1917.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXV. No. 4. Feb. 1918. 4to.
British Astronomical Association—Journal, Vol. XXVIII. No. 3. 8vo. Feb. 1918.
Buchanan, J. Y.—Naturhistorische Alpenreisi. By Hugi.
Über das Wesen der Gletscher. By Hugi.
Die Gletscher. By Hugi.
Carnegie Institute—Contributions from Mount Wilson Solar Observatory, Nos. 132–136. 8vo. 1917.
Chemical Industry, Society of—Journal, Vol. XXXVII. No. 2. 8vo. 1918
Chemical Society—Journal for Jan. 1918. 8vo.
Editors—
Aeronautical Journal for Jan. 1918.
Athenæum for Feb. 1918. 4to.
Author for Feb. 1918. 8vo.
Chemical News for Feb. 1918. 4to.

Editors—continued.
Chemist and Druggist for Feb. 1918. 8vo.
Church Gazette for Feb. 1918. 8vo.
Concrete for Feb. 1918. 8vo.
Dyer and Calico Printer for Feb. 1918. 4to.
Electrical Engineering for Feb. 1918. 4to.
Electrical Industries for Feb. 1918. 4to.
Electrical Review for Jan.-Feb. 1918. 4to.
Electrical Times for Feb. 1918. 4to.
Electricity for Feb. 1918. 8vo.
General Electric Review for Jan. 1918. 8vo.
Horological Journal for Feb. 1918. 8vo.
Illuminating Engineer for Dec. 1917. 8vo.
Journal of Physical Chemistry for Jan. 1918. 8vo.
Journal of the British Dental Association for Feb. 1918. 8vo.
Junior Mechanics for Feb. 1918. 8vo.
Law Journal for Feb. 1918. 8vo.
London University Gazette for Feb. 1918. 4to.
Model Engineer for Feb. 1918. 8vo.
Nature for Feb. 1918. 4to.
New Church Magazine for Feb. 1918. 8vo.
Observatory for Feb. 1918.
Physical Review for Dec. 1917. 8vo.
Power-User for Feb. 1918. 8vo.
Science Abstracts for Jan. 1918. 8vo.
Wireless World for Feb. 1918. 8vo.
Zoophilist for Feb. 1918. 8vo.
Franklin Institute—Journal, Jan. 1918.
Geological Society—Abstracts of Proceedings, Nos. 1018–1019. 8vo. 1918.
Imperial Institute—Journal.
Institute of Chemistry—Proceedings, 1917.
London County Council—Gazette for Feb. 1918. 4to.
London Society—Journal for Feb. 1918. 8vo.
Meteorological Office—Monthly Weather Reports for Feb. 1918. 4to.
Weekly Weather Reports for Feb. 1918. 4to.
Microscopical Society, Royal—Journal for Dec. 1917. 8vo.
New Zealand—Statistics for Dominion of, Vol. II. 1916.
Pharmaceutical Society of Great Britain—Journal for Feb. 1918. 8vo.
Photographic Society, Royal—Journal for Feb. 1918. 8vo.
Quekett Microscopical Club—Journal. 1917. 8vo.
Rome, Ministry of Public Works—Giornale del Genio Civile for Oct.-Nov. 1917. 8vo.
Royal Asiatic Society—Journal for Jan. 1918.
Royal Society of Arts—Journal for Feb. 1918. 8vo.
Sanitary Institute, Royal—Journal, Vol. XXXVIII. No. 4. Feb. 1918. 8vo.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXIV. No. 2, 1918.
Symons Meteorological Magazine—Vol. LIII. No. 25.
Tôhoku Mathematical Journal—Vol. XII. No. 4, 1917. 8vo
United Service Institution, Royal—Journal for Feb. 1918. 8vo.
United States Department of Agriculture—Journal of Agricultural Research, Dec. 1917-Jan. 1918. 8vo.
Experiment Station Record, Vol. XXXVII. Nos. 6–7. 1917. 8vo.
United States National Academy of Sciences—Proceedings, Dec. 1917.
University of Leiden—Communications from the Physical Laboratory. By Dr. Kamerlingh Onnes. Nos. 146–149, Supplement No. 39.
Western Australia, Agent-General for—Monthly Statistical Abstract, Nos. 199–204. 1917. 4to.
Zoological Society of London—Proceedings, 1917. 8vo.

WEEKLY EVENING MEETING,

Friday, March 8, 1918.

THE RIGHT HON. LORD RAYLEIGH, O.M. LL.D. D.Sc. F.R.S.,
in the Chair.

PROFESSOR EDWIN H. BARTON, D.Sc. F.R.S.

Vibrations: Mechanical, Musical, and Electrical.

I.—INTRODUCTORY SURVEY.

THE subject of vibrations is a large one. It comprises a great variety of to-and-fro motions, and these may be executed by diverse systems at widely differing rates. Near one border of the subject lie phenomena so simple that a child may grasp their leading features. Near the opposite border there are phenomena of exceeding complexity and their full solution is still awaited.

It thus appears that parts of the subject are too elementary and familiar for detailed treatment here, while others may be not yet ripe for general description. But between these extremes there are portions or aspects of the subject that may prove both interesting and practicable.

To indicate and locate a few such portions a brief survey of the subject was then taken. Many ways of classifying vibrations are available. But, without aiming at logical precision, a somewhat rough method was considered convenient. Thus, since a vibration is a to-and-fro motion, the various types of such motions may be placed in columns. Secondly, since these motions are executed by some physical systems, the various systems may be placed in rows or lines. This gives the sub-division shown in Table I.

Neither the columns nor the rows need stop just where they do in this table. For the subject extends further in each direction. Moreover, each column and row admits of further sub-division. So that the ramifications of the subject are almost beyond enumeration. But, as it is, it serves to locate the portions to which chief attention was directed. These were examples of two or more associated vibrations whether forced, coupled or compound.

II.—FORCED AND COUPLED VIBRATIONS.

Forced and *coupled* vibrations must be distinguished from each other and from the simplest class of all called *free* vibrations. To do this pass along the first row in Table I. taking the cases of the pendulums there shown.

If a pendulum bob is pulled aside and let go, it returns towards its zero position under the combined effect of gravity and its slant suspension. On reaching the zero position with a certain velocity it overshoots the mark because the bob has inertia. Thus a free vibration is set up. This may continue until slowly extinguished by friction which is operating all the time to diminish the swings. Next let the point of suspension of a pendulum be moved slightly to-and-fro by periodic forces. Then the pendulum would be set in vibration and kept going. Further, the motions would settle down to a quite definite amplitude and phase. These are the *forced* vibrations. Their amplitude would depend upon that of the point of suspension and also on the *tuning*. By tuning is meant the degree of agreement between the period natural to the pendulum and that of the forces applied to it. The closer the tuning between them the better the response. Upon the tuning depends also the *phase* of the forced vibrations. When the forces alternate appreciably slower than the vibrations natural to the pendulum the two are almost in like

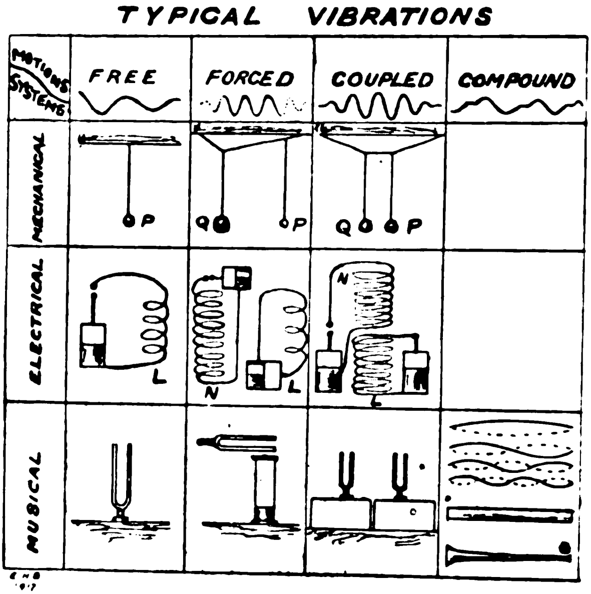

TABLE I.

phases. But when the forces alternate quicker than the pendulum, the latter swings almost in opposite phase.

This change of phase of forced vibrations was illustrated by three hanging pendulums all hanging from the same tightly stretched horizontal cord. One pendulum had a heavy bob and by its swings moved the stretched cord. It thus acted as *driver* and applied forces to the other two pendulums which had light bobs and so were easily *driven*. Of these pendulums one was shorter and one longer than the driver. They soon settled to opposite phases after the heavy bob was set in motion. *Resonance* curves showing the varied responses of such driven pendulums as the tuning is altered were then shown on the screen.

In the cases just dealt with the light bob is set in motion at the expense of energy taken from the heavy one. But on account of the great disparity of the bobs this loss entailed no appreciable diminution in the vibrations of the heavy bob or driver.

Consideration was next given to the case where equal bobs hang from a tight cord. While both pendulums are hanging at rest one bob is struck. Its vibrations disturb the other pendulum and set it in motion. But obviously, while the driven pendulum gains an amplitude equal to that first possessed by the driver, the driver itself would have lost all its motion. The other then becomes the driver in turn and transfers its energy back to what was originally the driver. (Demonstration).

This palpable surging of the energy to and fro between the two pendulums marks them as showing what may be called *coupled* vibrations. In both cases the action of the driver on the driven is recognised. But in the case of coupled vibrations the *reaction* of the driven on the driver is palpable and recognised also. Whereas in what are called forced vibrations this reaction is undiscernible or ignored.

In the case of coupled vibrations just shown the vibrations of each pendulum seem quite simple, but slowly and alternately wax and wane in amplitude, that is, they exhibit what are termed "*beats*." But it is well known that beats may be heard when two musical tones of slightly differing pitch are sounded together. Further, the number of beats per second is the difference of the frequencies of the two tones. Thus the waxing and waning vibrations of either pendulum may be regarded as the superposition of two simple vibrations of slightly different periods.

The next case studied was that of two precisely similar pendulums connected by hanging one from the bob of the other. One bob being started by a blow it appeared to execute simple vibrations. The other moved with a pause or twitch instead of in simple fashion. Further, neither pendulum showed the waxing and waning of amplitude which was so marked in the other case where both hung from a stretched cord.

The questions which now naturally arise are (*a*) why this contrast? and (*b*) can the gap be bridged? The solution is simple. The difference in appearance is only a matter of different ratios of periods of the superposed vibrations. And this again is due to different values of the *coupling*, to borrow a term from electrical theory. We have changed suddenly from a very loose to a very tight coupling. We consequently passed at a bound from periods nearly equal (giving a slow waxing and waning) to periods whose ratio exceeds 2 : 1 (involving the pause or twitch); for the theory shows that as the coupling increases the ratio of the periods increases also.

It is accordingly of interest to change the coupling gradually and so bridge the gap between the two motions which seemed so unlike. This was done by the cord and lath pendulum, in which the cord pendulum is suspended from an adjustable stud on the lath pendulum. When the two suspensions are near together the value of the coupling is almost equal to the fraction of the lath length at which the cord is attached. When this fraction is unity, as in the case of one pendulum hanging from the bob of the other, the coupling has the value $1/\sqrt{3}$ or 58 per cent. nearly. (These simple relations are for equal bobs and equal pendulum lengths.)

III.—ELECTRICAL VIBRATIONS, FORCED AND COUPLED.

On passing along the second line of Table I., it was noted how the various types of electrical vibrations may be obtained and the striking analogy to them presented by the mechanical cases already considered.

Any electrical circuit containing a capacity and an inductance may exhibit electrical vibrations. For the fundamental electrical conditions are there present just as the mechanical ones were in the case of a simple pendulum. If the condenser is charged by a suitable means, the quantity of electricity so displaced is urged to flow back again round the circuit by the electromotive force of the charged condenser. If the resistance of the circuit is small enough the electromagnetic inertia (measured by the inductance) ensures that the current shall still flow after the condenser is discharged. Thus its charge is reversed. So the vibrations continue till the energy is dissipated by the resistance of the circuit. These are *free* electrical vibrations.

As an example of forced electrical vibrations we may think of a circuit with capacity and small inductance (like that of a Fleming cymometer), placed not too near to a circuit of similar frequency but with much greater inductance. Then the cymometer will respond to the vibrations of the other, i.e. it will execute *forced* vibrations. These will not appreciably diminish the vibrations of the main circuit.

But let two electrical vibration circuits of comparable inductances and periods be placed together and started, then there is not only the action of the driver but also a distinct *reaction* of the driven on the driver. Hence, as the vibrations of one circuit start those of the other, the latter by their growth check the former, causing them to die away. Thus there may be an interchange of energy between them. This, as we have seen with pendulums, corresponds to the superposition of vibrations of slightly differing periods, provided the action and reaction are small and the interchange slow. Further, it is known that if two such circuits are closely coupled those two periods differ more widely. Hence a third circuit (say a cymometer) responding to either of them may detect these separate periods by giving a resonance curve with two humps instead of one.

IV.—TRACES FROM COUPLED PENDULUMS.

It has been seen that there is a certain general analogy between mechanical and electrical vibrations, whether free, forced or coupled. The question now arises as to whether this analogy may reach or approach a quantitative exactness in all or any respect, and whether it can be utilised in any way.

Various mechanical vibrating systems differ widely. Some resemble the electrical case very closely, but none appears to be completely and exactly analogous to them in every detail. Indeed the electrical case seems to be slightly simpler than any mechanical analogy yet put forward. But the differences are small, and the mechanical analogy may be highly useful as affording visible and tangible illustrations of those subtle electrical vibrations which can be neither seen nor handled. Especially is this the case if the model is readily adjustable to represent the various relations of the constants concerned and can be used for any initial conditions. Thus from such analogies some benefit may accrue to the non-mathematical student. But perhaps the highest advantage is realised only by those who combine the mathematical with the experimental study and grope after the ideal model which shall represent exactly the electrical or other phenomena in question. But whatever the uses of such models, certain it is that their design and study have exercised a fascination on many eminent scientists. In this connection it may suffice to mention Faraday, Maxwell, Lord Kelvin, Lord Rayleigh, Sir Oliver Lodge, Sir Joseph Thomson, Professors J. A. Fleming, T. R. Lyle, and W. S. Franklin.

For either quantitative work or mere illustration the usefulness of such a model is much enhanced if its vibrations leave traces. This is easily arranged by letting the bobs carry funnels of sand under which a blackboard moves uniformly at right angles to the direction of vibration. In the portable apparatus shown in Fig. 1 the pendulums are of the double-cord type and allow both traces to be obtained simultaneously, and thus record the relations of amplitude and phase for each pendulum. With this apparatus the coupling can be varied at will and easily adjusted to any desired value from 1 per cent. to 60 or more. The greater the droop of the bridles the greater the coupling, the quantitative relation being simple. It is noteworthy that, for equal bobs and pendulum lengths, a 60 per cent. coupling gives superposed periods as 2 : 1, just as in the electrical case for equal periods. Indeed, with any specified coupling, the ratio of periods is the same for this mechanical case and for the electrical one. The masses of the bobs and the lengths of the pendulums are adjusted at pleasure, and the initial conditions may be anything that is desired. (Simultaneous traces with this apparatus were then obtained, others exhibited, and photographs of a number thrown on the screen.)

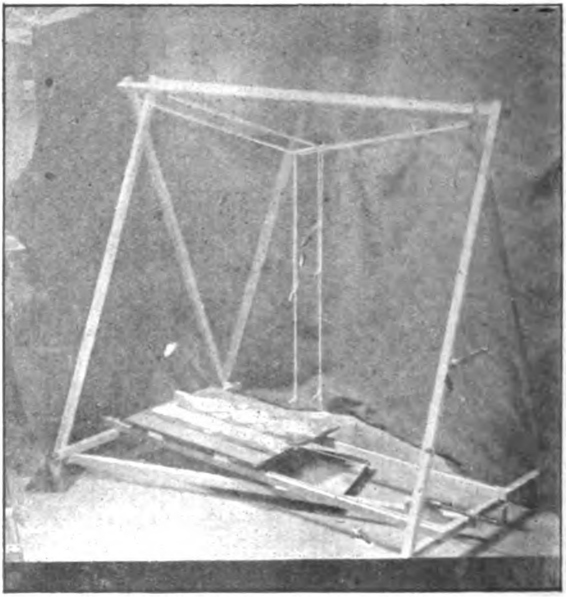

FIG. 1.—COUPLED PENDULUMS.

With equal bobs and equal lengths, the coupling being small, each pendulum exhibits in turn the same maximum and the same minimum as the other. With small couplings, equal lengths, but bobs as 20 : 1, the case of forced vibrations is approached. That is

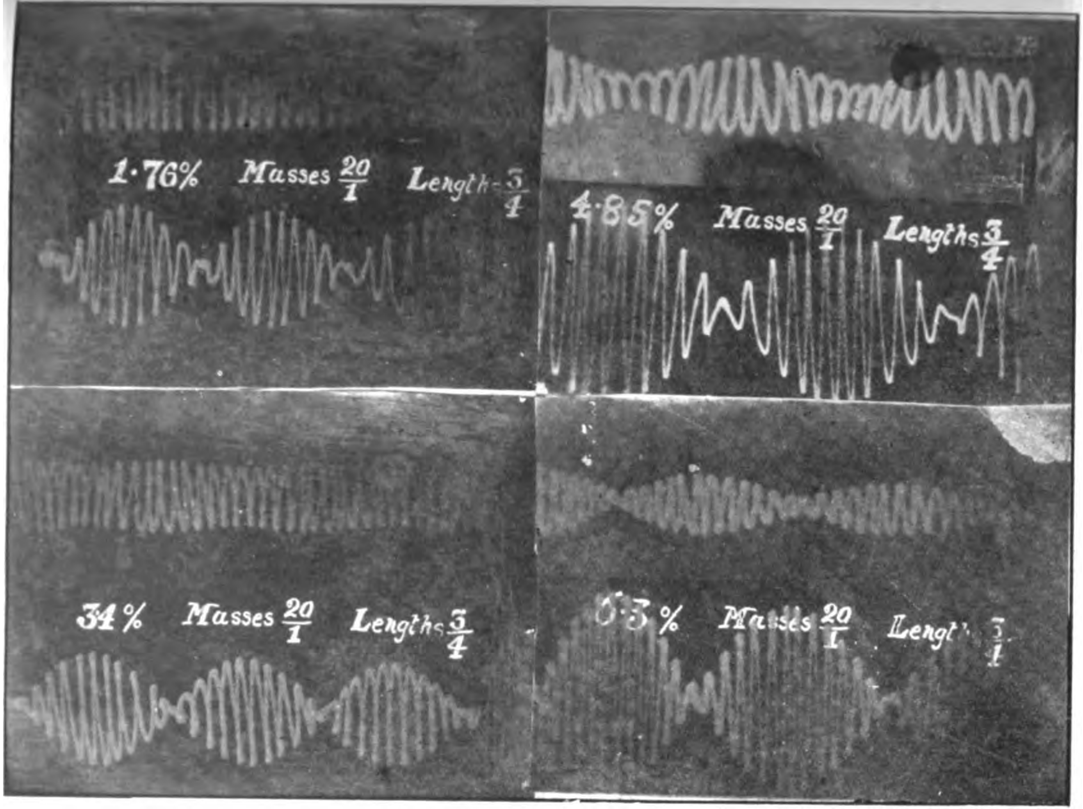

FIG. 2.—VIBRATION TRACES OF COUPLED PENDULUMS.

to say, the heavy bob loses but little amplitude, while that of the light bob grows from zero to its maximum. With bobs as 5 : 1, the heavy bob loses appreciably, while the light one proceeds to its maximum.

As the coupling increases from zero the ratio of the periods of the superposed vibrations of the coupled pendulums usually increases continuously till it equals or exceeds 2 : 1. When, however, both lengths and masses are unequal, the *short* length having the *heavy* bob, a new feature appears. As the coupling gradually increases from zero, the ratio of the periods at first diminishes, reaches a *minimum*, and then increases. Thus the number of vibrations in a beat cycle at first increases, reaches a *maximum*, and then decreases. These special effects are shown in Fig. 2. They were theoretically predicted and then experimentally confirmed. The maximum number of vibrations in the beat cycle occurs for the highest coupling shown in the figure, viz. 6·3 per cent. The details as to bobs, lengths and couplings are all indicated on the figure. The able collaboration of Miss H. M. Browning in this work was gratefully acknowledged.

V.—BRASS INSTRUMENTS AND THE LOW "F."

Leaving the pendulums which have only two vibrations at a time, the case of brass instruments with a number of simultaneous vibrations was next considered. It is well known that the vibrations from most musical instruments are what is called compound. They consist of a series of tones of commensurate frequencies sounded together. Thus, if the pitch of the note is said to be 100 per second, there is not only a prime tone of this frequency, but also a second tone of 200 per second, a third of 300 per second, and so forth. This law applies to strings, to open parallel pipes, and to a complete cone with its base open. It also applies as a close approximation to the brass instruments in general use. This approximation is traceable to the departure from the strictly conical form as regards the mouthpiece, the bell, and the special shape of the intermediate portion.

In these brass instruments the possibility of this compound tone, or multiple resonance, is utilised for the production of distinct notes. Thus, out of the tones possible to the instrument the player may elicit the set 200, 400, 600, 800, etc.; or the set 300, 600, 900, 1200, etc. These would be said to have the pitches of their primes, or lowest components 200 or 300 respectively. Or, to put it musically, they would be the octave or the twelfth of the fundamental (or pedal) possible on the instrument. The pedal of the instrument is not usually employed for musical purposes, but can be sounded if specially wished. Now, there is a tradition among players of brass instruments that a note called by them a low "F" can be sometimes obtained. This note would have on the foregoing scheme the frequency $133\frac{1}{3}$. At first the possibility of this "F" seems scarcely credible to the theoretician. But after hearing and producing the note, the necessity of accounting for its possibility was forced home.

And really the explanation proves very simple. It usually depends upon two points; (*a*) the *spread* or *diffused resonance* of the pedal; and (*b*) its intentional *mistuning* with respect to the other notes of the instrument. These are taken in order.

(*a*) For theory shows that, other things being equal, the lower the note of such an instrument, the easier it is to force its vibrations out of tune, sharper or flatter. Thus, with the pedal the range of resonance is such that the note may be sounded at any pitch whatever over a range of five or six semitones.

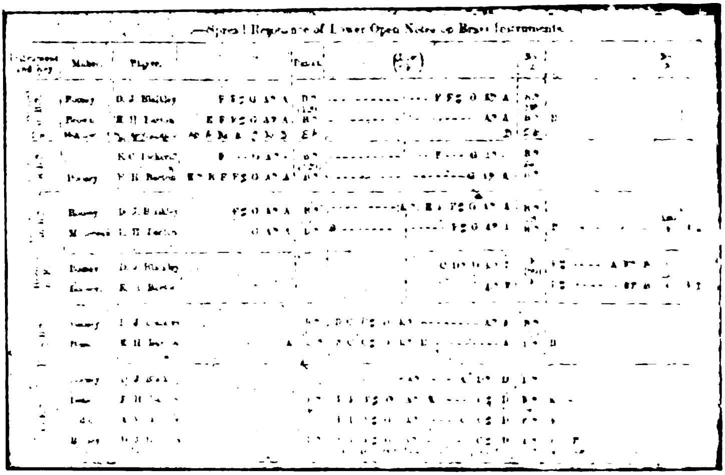

TABLE II.—Spread Resonance of Lower Open Notes on Brass Instruments.

(*b*) Since the law of frequencies 100, 200, 300, 400, etc., is only approximately true for these instruments, in order to secure good relative tuning of the higher notes which are in constant use the pedal (which is not used musically) is purposely mistuned. On some instruments it may be say, D or E flat, instead of C.

Hence, if the central pitch of the pedal is sharpened two or three semitones, and it is possible to force this note both up and down two or three semitones, it becomes possible to sound the pedal of true pitch C, to sound the low "F," and to sound notes of every pitch between. (This was demonstrated on a euphonion kindly lent by Messrs. Boosey & Co.)

The low "F" is also possible on the bombardon. Both these instruments are characterised by large conical tubing, and the low "F" is obtained by the spread resonance of the sharpened pedal.

In the case of the trumpet, cornet and French horn with much narrow tubing, the pedals are flattened, so that a pedal of true pitch can only be obtained by the spread resonance, and the "F" is impossible. On the trombone, which has much small parallel tubing, the low "F" may be obtained occasionally by the downward spread resonance of the second partial (or note number two), which is an octave above the pedal. (Demonstration.) The pitch of the notes which have been obtained on six types of instruments by four experimenters are shown in Table II.

VI.—MONOCHORD VIBRATIONS.

Consideration was next given to the vibrations of stringed instruments, beginning with the monochord, because of its striking simplicity.

From the work of mathematicians (with a little help from experiment) the various possible vibrations of strings, whether plucked, struck or bowed, have long been well known. But a little reflection will show that many other problems are still left confronting the physicist. For identical strings excited in the same way, but mounted on different instruments, will produce very different effects on the ear. In other words, the worth of a violin does not lie in its strings, but in its sound-box.

This leads to the inquiry as to what happens to modify the vibrations as, passing from the strings, they reach in turn the bridge, the belly (or sound-board) and the adjacent air.

It is easy to see that this problem is somewhat complicated, since it presents so large a number of variables. Thus, there lie at the experimenter's disposal the pitch of the string, its material and dimensions, the place and manner of excitation, the material and disposition of the associated parts of the instrument, the place of observing the belly, the portion of the bridge observed and the directions of its motions, and, lastly, the spot at which the motion of the air is observed. In this way a scheme for over a thousand observations could be sketched, even for an instrument with but one string.

Hence no exhaustive treatment of the problem can be quickly obtained. But a beginning has been made, and by very simple means.

In a series of experiments simultaneous records have been photographically obtained of the vibrations of the string and of some other part of the instrument. The monochord was placed on a table and light from a vertical slit was focused upon the string near its centre. The real image of this slit, crossed by the shadow of the string, was then focused by a second lens on to a photographic plate in a dark room. This plate was shot along horizontal rails by elastic cords which were just slack when the plate received the light. Thus the plate moved uniformly and horizontally, while the shadow of the vibrating string showed its special motion vertically. The corresponding motions of bridge, belly, or air were obtained on the same photographic plate by the light reflected from a tiny rocking mirror whose slight tilt was produced by the motion of the part under test. (The principle of this experimental method was then demonstrated, the humped form of the curve due to plucking the string and the two-step zig-zag produced by careful bowing being shown.) Fig. 3 gives a diagram of the method for the monochord, also a detail of the rocking mirror for the bridge's motion. Fig. 4 shows photographic traces for the monochord string and belly. The two curves

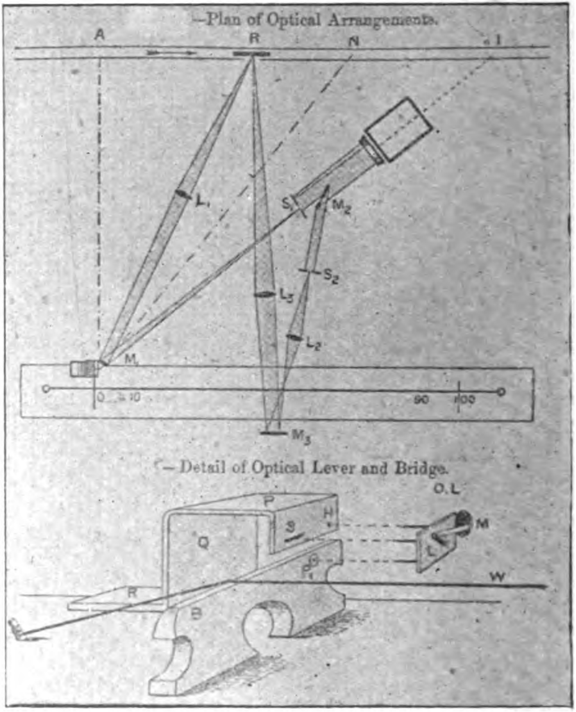
—Plan of Optical Arrangements.
A
R
N
1
L₁
S₁
M₂
S₂
L₃
L₂
M₁
0
10
90
100
M₃
—Detail of Optical Lever and Bridge.
O.L
P
H
M
S
Q
L
B
P₁
W
R
B

FIG. 3.—MONOCHORD APPARATUS.

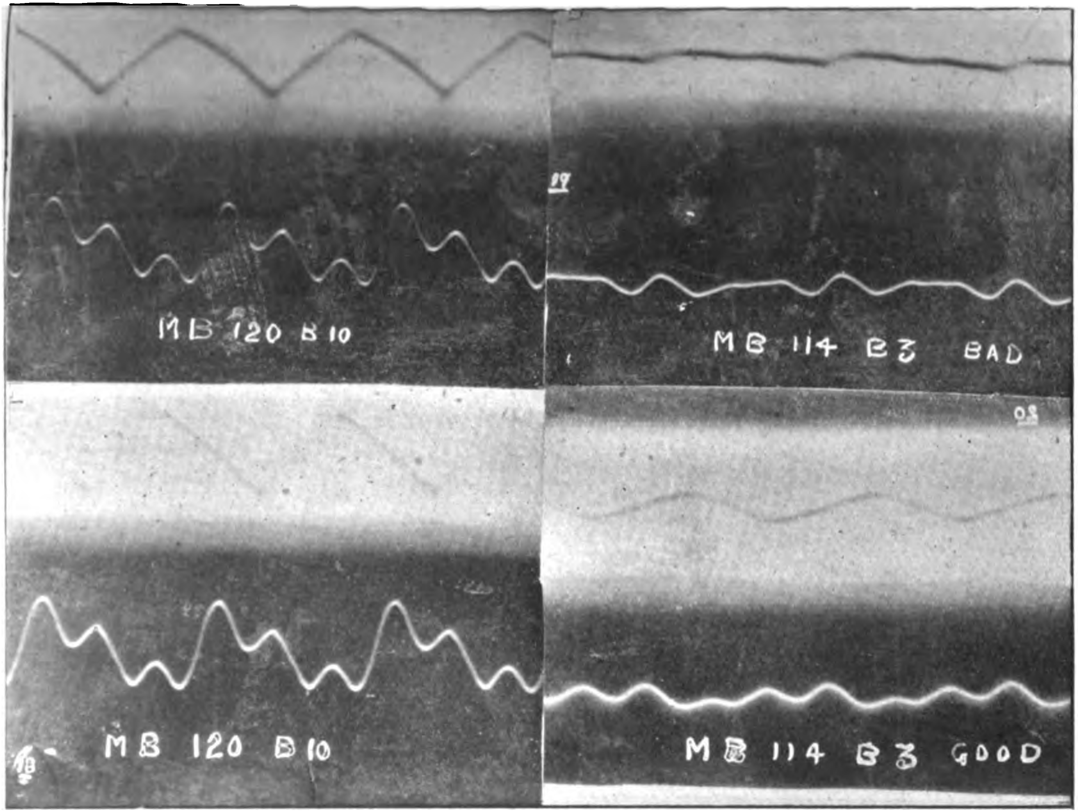

FIG. 4.—MONOCHORD VIBRATION CURVES.

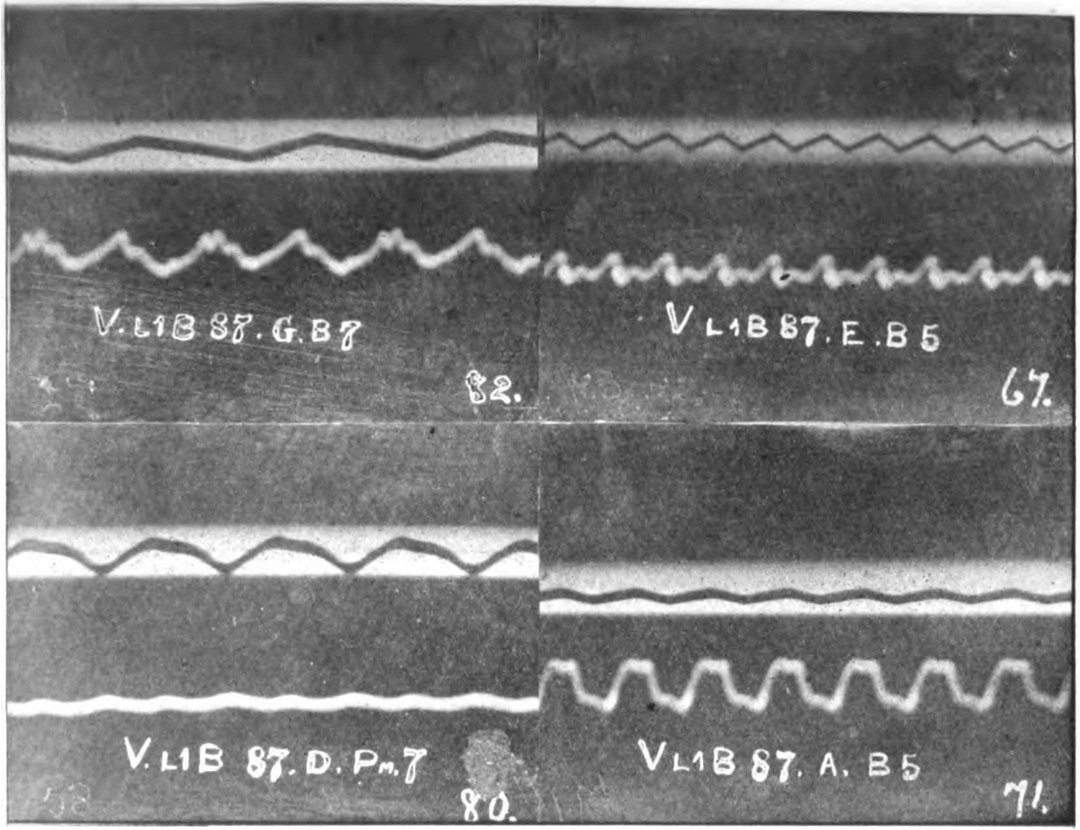

FIG. 5.—VIOLIN VIBRATION CURVES.

alike were taken separately to test if the apparatus worked satisfactorily. The other two curves, slightly different from each other, show the distinction in appearance between the records of a bad tone and a good one.

In this work the assistance of Messrs. C. A. B. Garrett and J. Penzer was acknowledged. In 1914 Prof. C. V. Raman, of Calcutta, by experiments somewhat similar to the above, showed that the forward speed of a string where it is bowed is identical with that of the bow itself.

VII.—VIOLIN VIBRATIONS.

If the problems of the monochord were numerous and complicated, those of the violin are still more so. For there are now four strings instead of one; further, all are different in thickness and pitch, and are capable of use in sections of varying length. Again, the soundbox is curved in a variety of ways. Finally, the reinforcement of the belly is asymmetrical. The bass bar lies almost under the fourth string, while the sound-post stands near that foot of the bridge which is under the first string. In the work on the violin assistance was received from Messrs. T. J. Richmond, T. F. Ebblewhite, and W. B. Kilby. A number of vibration curves obtained for the violin were shown on the screen. Fig. 5 gives one set of these, showing the vibrations of each string as indicated by the letters G, D, A and E. The D string was plucked by a sharp point, the other strings were bowed. The white line shows the longitudinal motions of that corner of the bridge near which the first, or E, string passes.

VIII.—CONCLUSION.

With respect to the sympathetic vibrations occurring in stringed instruments, it is obvious that though a little has been done much more remains awaiting attack. Thus, the violoncello, guitar and harp might be dealt with. But specially, because of its immense vogue, the pianoforte needs thorough investigation. A start was made some time ago by Mr. G. H. Berry, and further researches are now in progress in London under the joint direction of scientists and piano manufacturers.

In the past music lovers and scientists alike have been deeply indebted to the makers of musical instruments, who have themselves received but little help from science in return. The lecturer expressed the hope that science might shortly pay off part of its debt to the musical craftsmen of the country and help to make the British piano second to none in the world.

[E. H. B.]

WEEKLY EVENING MEETING,

Friday, March 15, 1918.

J. H. Balfour Browne, K.C. LL.D., Vice President,
in the Chair.

Right Rev. W. Boyd Carpenter, K.C.V.O. D.D.

The Romantic Revival.

It is always interesting to note the waves of fashion as they rise and fall amid the tides of the world's progress. I am to speak to you about the Romantic Revival of English Literature. Its story may be taken as measuring an incoming tide, but the surface of the movement is broken by the personal animosities and prejudices: these are as the noisy waves, which by calling our attention to themselves make us forget or ignore the power of the tide. I cannot hope to discriminate fully and fairly between the majestic rush of the waves and the silent power which works unseen beneath them; but the evidence of the power of the Romantic movement will, I hope, be apparent to us as we proceed. Perhaps two anecdotes here will serve to illustrate both the force of the movement and the prejudices against which it had to contend. The first of these illustrates the ascendancy of the movement over the active minds of the 19th Century. The late Professor Courthope has given us his opinion that from one point of view Byron is "the most complete representative of the romantic movement in English poetry" (Hist. of English Poetry, vol. vi. p. 274). Here is an example of its power. One day the late Lord Beaconsfield said to Lord Rowton, "Monty, it takes a hundred years to breed a poet; and Byron is the poet of the 19th Century." Mr. Disraeli was born in 1805. He was in his boyhood when the First Canto of "Childe Harold" had made Byron famous, and his young days were brought under the spell of the extraordinary popularity of "The Bride of Abydos," "The Corsair," and the later cantos of "Childe Harold," and of the fascination which the restless and enigmatic character of the poet exercised over those whose hearts responded to the call of liberty. Though other poets greater than Byron arose, Lord Beaconsfield never outgrew the prepossessions and memories of his youth

The second anecdote shows the strength of the prejudice against which the Romantic movement had to contend. Though there were enthusiasts who welcomed the younger poets, reverence for earlier bards did not disappear. Delight in the Classic age never wholly died. The French painter De la Roche prided himself on his Classical severity. He was at work one day upon a picture, when the amiable and accomplished secretary of a French society came and watched him at his work, and after a silence of observation, exclaimed, thinking to compliment him, "You are the very Victor Hugo of painters." The painter flung down his brush in disgust. He had no wish to be compared with a leader of the Romantic school. There were many in the world of letters who felt like the French painter The love of the Classical age did not die in England any more than it did in France; but the new school, in whose atmosphere the old among us spent our younger days, reached a pre-eminence and popularity which changed the current of thought and of what is perhaps more powerful—of taste. Let me imagine that a student has strayed into a library. Let me suppose that he has no knowledge of the rival claims of Classical and Romantic writers. He turns to the long rows of bound volumes of the Reviews. He takes down a volume of the *Quarterly* or the *Edinburgh* and reads a criticism on the writings, we will say, of Scott or Wordsworth. He learns that in the judgment of the reviewer Wordsworth has little claim to the respect of the public or the title of a poet. I give you one or two specimens of what he might read—indeed, of what you may all read if you were minded to spend an hour or two among old Reviews. Of Scott we read that he has "a tone of animation, unchecked by any great delicacy of taste or elegance of fancy." The reviewer regrets that a writer of such talents should "consume them in imitation of obsolete extravagance. To write a modern romance of chivalry is like building a modern abbey or an English pagoda. There is little connected incident and a great deal too much of gratuitous description" (*Ed. Rev.*, 1808). Of Wordsworth the same Review fourteen years later says: "The Lake school of poetry we think is now pretty well extinct. Wordsworth has fallen into a way of writing prosy, solemn, obscure, feeble kind of mouthing, sadly garnished with shreds of phrases from Milton and the Bible, but without nature and without passion, and with a plentiful lack of meaning, compensated only by a large allowance of egotism" (*Ed. Rev.*, 1822). Our reader is tempted to think that it will be but wasted time to spend thought or study upon a writer who is thus described. But the articles are piquant, and he would like to hear a little more. He takes down another volume of the same Review, and now he reads criticisms of a wholly different strain. A man called Wordsworth is now declared to be a poet of no mean order. His principle of pure diction has caused the expurgation of absurd terms. The nightingale is no longer Philomel or the tuneful bird of night.

Our reader rubs his eyes. Can this be the same poet? If so, how has cursing turned to blessing? Is Balaam also among the critics? And what interesting creature has led him to change his mind? Our reader examines more carefully. The poet is the same. The dates of the Reviews are now examined. The dates of the abusive article are in the early part of the century; the kindly and laudatory articles are less than a generation later. The reader is more perplexed than before. Is there any fixed canon of poetry? he asks. Was that good poetry in 1834 that was vile stuff in 1824? If opinion can jump about in this style, how can any simple student know how to direct his steps? Poetry has been an art for thousands of years. Do we know so little about it that we cannot apply any fixed standard to estimate its value? Is there no golden rod by which we may measure it?

Here we open up a question too great to answer; my time this afternoon, my capacity at any time, is, I think, not equal to the task. Nevertheless, lest you should think me cowardly, let me say one word on theoretical conceptions of poetry. I have found them one and all to be wanting. They may be summarized in this way. Some give us a vague definition which includes too much; some, on the other hand, attempt the would-be exact definition which usually excludes some essential quality; and, lastly, there is the descriptive attempt, which of course does not define at all.

When Milton tells us that poetry should be simple, sensuous and passionate, he speaks of qualities which we can probably admit are qualities indispensable to good poetry; but he does not define poetry any more than you define a man by telling us that he must have form, reason and volition.

When Mr. Swinburne tells us that poetry should possess imagination and harmony, we quite assent, but we feel that we are no nearer a definition of poetry.

As one who has expressed a dissatisfaction with attempted definitions, perhaps I may be allowed to go further and express, with much hesitation and deference, the doubt whether any true and real definition of poety can be given. Let us put this in another way. Can we ever define that which is in its nature beyond science? Within the limits of science we may and we must define; but there are regions in which science is a stranger. Science walks on two feet; but imagination has wings, and science, with rule and compass, cannot follow her in her flight. We know that poetry, like imagination, has eyes which can pierce beneath the surface of things, and wings that can lift her heavenward. But when we try to hold her fast that we may make an inventory of her dimensions she escapes from us. That which has its home in heaven must ever elude the grasp of earth.

But if some demur to this view, perhaps they will be more in agreement with me when I say that the definitions we quoted were given by poets, and if anyone can give a definition of poetry, least of all can the poet do so. The man of science may tell us how a thing is done; the artist seldom can. He may give us some information about certain of the minor or more mechanical parts of the processes in work; but the true secret of his art is hidden most of all from the artist. Once I visited Mr. Tinworth in his studio. I asked him something about his process of work. He took up a piece of clay, and under his nervous and pliant fingers, which seemed to have life and reason in their very tips, the tiny piece of clay took human shape.

"That is how I do it," he said, as he crushed it into shapelessness again. Yes, that is how we do it, the poet as well as the artist tells you; but the *how* is as much a mystery as ever. The artistic and the poetic mind is bad at definitions, and therefore when Matthew Arnold said that poetry was at root a criticism of life, we take the liberty of believing that he is projecting the shadow of something which profoundly interested him over his idea of poetry and mistook the outline of the shadow for a definition of poetry.

Indeed I would go further—and this has intimate bearing on the rise of the Romantic School of poetry—and express my belief that a poet never writes so badly as when he is under the conscious rule of some theory of his art. He is only master when he rises so far above it (I do not say neglects it) as to be unconscious of its existence; for there is always in true poetry a certain happy abandon which tends to create that the quality of "inevitableness" of which Wordsworth spoke.

Briefly, then, I distrust definitions, but this is quite a different thing from saying that there are no qualities which should be found in poetry. We look for imagination and harmony; we delight in simplicity where we find it; we ask for colour, and we love it as it appeals to our sense of fitness. But we must give poetry a latitude which is claimed rightly by all imaginative arts; and we must allow to the poet the freedom of his individuality.

He must, as has been said, create his own public. He must be true to himself if he is to be true to those souls of whose secret thoughts and unspoken aspirations he is to be the voice.

When, therefore, our reader in the library asks whether that is good poetry in 1830 which was bad a few years before, our answer must be that between the two dates there had arisen a generation who found in Wordsworth the voice for which they were waiting. In a part he created the thirst which he satisfied; in a part he was the mouthpiece of what struggled in many souls for utterance. The age had changed. Wordsworth suited the England of 1830; he did not suit the England of 1810–20. And whatever else is true, the poet must speak the language of his age. The age says of the poet, as the lover does of his mistress,

> "If she be not fair to me,
> What care I how fair she be."

And this sentiment must not be thrown away as though it only meant that the taste of one age is bad and that of another is good. This may be true; but if the poet has a message for the men of his time he must deliver it in a language understanded of the people. That which is good may not be fit. Some few years ago, a preacher of well-known elocutionary power made the experiment of reading in his church the famous sermons of famous divines. The experiment was a failure; but nobody therefore will argue that Latimer's sermons at St. Paul's Cross, or Jeremy Taylor's sermon on the "Wedding Ring," were not great sermons. They were good, but they did not appeal to the men of our own day whose fashion of thought was not that of the 16th and 17th Centuries. Thus it was with Wordsworth; he did not appeal to the men of 1810–12, but he had won his way to the hearts of the Englishmen of a later decade in the century. He won his way, not by changing them, but because his was the harbinger voice of a great change which had passed over them.

Here we touch the contrast between the 18th and the 19th Century.

We have heard much said against the 18th Century. I do not know any century for which I have more sincere pity did I not know very well that the century stands in no need of pity. There have, however, been those who could see no good in it. It is the dead century. It had not intelligent politics, noble art or living religion. I deprecate such language. It is neither true nor useful. One thought may assure us of this. Some one said that the first duty of a book was to be interesting. If it had every virtue and lacked interest it must be described a failure. Now the 18th Century may be supposed to lack every virtue in art, politics or faith; but it retains the virtue of being pre-eminently interesting.

In truth, in spite of such disparagement, the men of the 18th Century interest us far more than the men of the 19th; and there are few of us who would not prefer to join a Reunion at Leicester Square with Joshua Reynolds, where we should be sure to meet as fellow-guests Dr. Johnson with his trenchant thunder; Boswell, alert, observant and obsequious; Garrick, with his provoking attractiveness; and last, but not least, the two immortal Irishmen—Edmund Burke, with his vigorous mind, his copious eloquence, and his incorruptible principle; and the sweet, rare, pathetic genius of his age, Oliver Goldsmith—than have been present at one of Roger's parties or joined the wits at Holland House, even though Macaulay or Sydney Smith might be there.

It was an interesting century. It gave us a galaxy of great men who are not yet dead, and who can never die, because a fairy had kissed them at their birth.

I do not mean merely that their works are on our shelves, and that they belong to the class which are needful in every gentleman's library. I mean that there were men in the 18th Century whom we recognise as magnificent personalities to-day. And what statesman to-day is not happy when he can shelter himself under the ægis of

Edmund Burke? Every playgoer is pleased when the "School for Scandal" is put on the stage. We take an increasing interest in Dr. Johnson's table-talk. We have enthroned in our spiritual imagination both John and Charles Wesley; and a great and venerable statesman of our own day dedicated a considerable portion of his life to editing and commenting on the works of a great 18th Century divine, Bishop Butler.

The century had its weaknesses. It formed theories, and it rode its theories to death. It had its heroes, and it sought to raise its heroes to heaven. It may have even worshipped those who were no gods. We may grant the absurdities of the century. It loved the heroic couplet; and for this I hope we shall not blame it. It is a noble vehicle of poetic expression; but we may wonder—and if we are fretful we shall be angry—when we hear Atterbury urging Pope to take up Milton's "Samson Agonistes" and improve it, and naively suggesting that it is really worth while. "Some time or other I wish you would review and polish that piece . . . It deserves your care, and is capable of being improved with little trouble into a perfect model and standard of tragic poetry."

We can understand a revolt against the tyranny of the heroic couplet, when William Hamilton (author of the "Braes o' Yarrow," 1704–1754) takes "Hamlet" in hand and gives us the following, we suppose as an improved version of the soliloquy:—

"My anxious soul is tore with doubtful strife,
And hangs suspended betwixt death and life;
Life! Death! dread objects of mankind's debate!
Whether superior to the shocks of fate,
To bear its fiercest ills with steadfast mind,
To Nature's order piously resigned,
Or, with magnanimous or brave disdain,
Return her back the injurious gift again."

But these follies were not shared by the greater men. They might have their preferences and their own methods; but they did not fall under the yoke of bondage, or believe that a man can be great by rule, or even great without having a spirit above rule. Pope, in one sense the cheftain of his school, perceives this clearly enough, and gives it utterance:—

"Some beauties yet no Precepts can declare,
For there's a happiness as well as care.
Music resembles poetry, in each
Are nameless graces which no methods teach,
And which a master-hand alone can reach,
If, where the rules not far enough extend,
(Since rules were made but to promote their end).
Some lucky Licence answer to the full
Th' intent propos'd, that licence is a rule.
Thus Pegasus, a nearer way to take
May boldly deviate from the common track.
From vulgar bounds the brave disorder part
And snatch a grace beyond the reach of art."

This passage sufficiently vindicates the freedom of the poet. It may, however, be questioned whether the standard taste of the age of Pope would have tolerated the liberty which, in theory, the poet claimed for his order. "King Alexander," as Henry Fielding called him, put "a total restraint on the liberty of the press: for no person durst read anything which was writ without his licence and approbation; and this licence he granted only to four during his reign, viz. to the celebrated Dr. Swift, to the ingenious Dr. Young, to Dr. Arbuthnot, and to one Mr. Gay, four of his principal courtiers and favourites."

The tendency of the prevailing taste was to look askance at the innovator. The result was the growth of an unwritten code of taste. Such is bound to become a burden which ardent souls will find too heavy to bear. Meanwhile, new ideas have taken possession of men's minds. Men's thoughts cannot find natural expression in the old forms. The new ideas demand new vehicles of utterance. The new wine must be put into new bottles. Sooner or later the revolt must come; and when it comes, the virtues of the old are forgotten in resentment against its real or apparent tyranny. The old regime must pass away. The dear quaint figure, trained to many courtesies, making love by rule, powdered, precise, well-disciplined in discreet graces, is to give way before freer manners and less conventional costume. Taine recalls that when M. Roland appeared before the King without the Court costume, someone said, "All is lost." But as Taine truly remarks, it was but the symbol that all was changed. The age of refined, even superfine, cultivation, of careful observance of ancient forms, is at an end. The powder-box and the pig-tail disappear. The 19th Century and sans-cullotism are at the door.

We make our bow to the new-comer, and we try to become acquainted with his features. He embodies the spirit of Revolt. What is the revolt which he expresses? New influences are at work. Three great factors have been at work in Society; and all of them are handmaids of change—the revolution, machinery, and missionary enterprise. They are all movements of and for the people. The Revolution will proclaim the Rights of Man; machinery will popularise the products of the world; missionary enterprise will make its protest on behalf of forgotten and despised races. To those who have eyes to see it, these movements mean that the treasures of life will no longer be the monopoly of the few.

The singer will no longer sing to please his patron. He has heard the murmur of those voices which are as the waves of the sea. He thinks no longer of some Lord Chesterfield, or of Dr. Johnson. He may wince under the lash of Jeffreys; but he will appeal from the judgment of the *Edinburgh* to that of the public, and the public is no longer the few who are cultivated; the public is to him the masses, whose lives are sad and whose hearts he longs to reach. He is moved by a higher conception of his calling; he feels that the singer should be a seer; the people need a guide and a voice. May not his art be a voice and guide to them? Does he live only to sing a new song in the people's ears? He may have but a small provision, but if the people are hungering, he will spread forth his scant fare in the wilderness. It will be something if he can appease their hunger. It will be reward enough if he can give a new song to the new generation.

> "And here the singer, for his art
> Not all in vain may plead,
> The song that stirs a nation's heart
> Is in itself a deed."

I do not mean that thoughts such as these rose consciously in the minds of the singers of the new poetry; but I do mean that the movements of the age made the idea of the people clearer, and more living and more operative to those whose minds were open to the meaning of the lightning and thunderings and voices which accompanied the close of the 18th Century.

The literary movement was a popular movement. It was also a revolt against conventionality. It was a plea for liberty.

Any custom may after a time become a tyranny; and this not because it is bad, but because, as Tennyson reminds us, even a good custom may corrupt the world. The cry for change may be a puerile or a manly cry. It may be the cry of one who is heedless and inattentive, and who is as a child wanting a new plaything because it has not the energy or intelligence to get amusement or interest out of the old. On the other hand, it may be the cry of the builder who is asking straw to make his bricks. Life shows itself to be life by its resolute selection of the form in which it can best express itself. Life governs form far more than form governs life. Wherever there is true life there will be a certain majestic independence and determination to live its own life. And here an 18th Century worthy will support me. "No man," said Dr. Johnson, "ever was great by imitation."

The fault of the old generation had not been, as some have said, that they were hopelessly prosaic. I cannot agree that there is any fitness in calling Dryden or Pope "prose writers." You may assign them any rank you please, high or low, in the Pantheon, but you cannot turn them out of doors and place them in company with Miss Burney and Miss Edgeworth. If Pope is not a poet, who is? is a fair question, if we understand it rightly. Pope does not attain to the rank of those who sit in the snow-crowned heights undisturbed by the storm of criticism which rages round the lesser heights; but if we are to count every poet a prose writer who is prosy we shall have few poets left. There are traces of prosaic wilderness enough in Wordsworth as well as Pope, but as we cross them we can hear the murmur of the burn, and we know that we shall before long descend into a green glade, softened in sweet shadow and musical with running water.

The fault of the century lay rather in the fastidiousness of taste which became too burdensome for Nature. The age was too determinately genteel. The writers were afraid to laugh—they only simpered; they gently pressed the delicate cambric across their eyes, because it was not good taste to show emotion. "Fear of being vulgar, fear of being singular, these were the real nightmares that sat upon 18th Century poetry."

Against this conventionality the new school revolted. It claimed the right to be natural. It entered once more into closer connexion with the world of Nature. It no longer treated Nature as a beautiful thing, but a thing outside—a treasury of images and tropes. It sought to get into the heart of Nature. It loved simplicity. As was to be expected, it overdid it. In striving for simplicity it sometimes assumed a simplicity like that of the dear little country-girl on the stage, who is artfully simple, who is self-consciously straining after naturalness of manner, and whose affectations are contradicted by the elaborate daintiness of her sham rural costume.

Are Wordworth's little girls and peasants always truly natural? Do they not sometimes look up to us with a certain theatrical pertness? The very effort to avoid the conventional betrays people into another kind of conventionality. And here I may make a remark which you will laugh at as quite absurd. The only way of writing naturally is to be natural while you write. The poet who has his theories too fixedly in his mind as he writes will miss that happy naturalness which only comes to those who do not look for it.

But nevertheless the determination to be natural and simple was good. There were things which needed to be done, and the world was going to do them. There were things which needed to be said, and men rose up to say them. We shall no longer hear Pope or Addison or Goldsmith. Instead we shall hear Collins and Gray, Byron and Shelley. The heroic couplet will give place to a freer fashion of verse. The ponderous novels of Fielding and Richardson will be superseded by the ardent romances of Sir Walter Scott. And even the most devoted admirer of the Classic age will admit that English literature has gained by the movement.

I am not here to discuss Wordsworth's theories of poetry. I have said—what I believe—that a poet's theories of poetry are probably worse than a critic's, and that Wordsworth did his best work when he left his theories alone and wrote in happy abandonment and forgetfulness of his self-made rules. But whatever be the value, or want of value, of these theories, the movement which is called Romantic was the dawn of a period of song which was sweeter, full-throated, more varied, more widely appealing than any which had been heard for a hundred years.

I decline to enter into the question whether Pope was a greater poet than Wordsworth, or Dryden than Tennyson, or Shelley than Gray. These always seem to me to be vain and profitless comparisons. When I go into a garden I do not argue that a rose is nobler than a lily, or a daffodil than a crocus. I bless each flower as it comes. Its beauty and its fragrance are its own. Each has its season, and in their season all are welcome. And in this fashion I can enjoy the rich banquet which the Romantic School has spread for us.

The song which Burns wrote on New Year's Day more than a hundred years ago expresses the popular side of the movement, and has graven it into the souls of every generation since :—

"The rank is but the guinea's stamp,
The man's the gowd for a' that."

When "John Anderson my Joe" has been sung many a "frosty pow" has stolen unseen across the sofa and clasped a thin white hand upon which the worn wedding-ring hangs loosely.

No generation, least of all our own, can afford to forget the lesson which Wordsworth taught when he sung :—

"The world is too much with us; late and soon,
Getting and spending, we lay waste our powers;
Little we see in nature that is ours;
We have given away our hearts, a sordid boon!
The sea that bears its bosom to the moon;
The winds that will be howling at all hours,
And are up-gathered now like sleeping flowers;
For this, for everything, we are out of tune;
It moves us not. Great God! I'd rather be
A Pagan suckled in a creed outworn;
So might I, standing on this pleasant lea,
Have glimpses that would make me less forlorn;
Have sight of Proteus rising from the sea;
Or hear old Triton blow his wreathed horn."

But it is in no single passage that we can fairly measure the power of any writer. "Not," says Mr. Myers, "not the isolated expression of moral ideas, but their fusion into a whole in one personality is that which connects them for ever with a single name. Therefore it is that Wordsworth is venerated; because to so many men—indifferent, it may be, to literary or poetical effects as such—he has shown by the subtle intensity of his own emotion how the contemplation of Nature can be made a revealing agency, like Love or Prayer, an opening—if indeed there be any opening—into the transcendent world."

And when we remember Him who said "Consider the lilies," "Consider the ravens," we may well feel how much of larger thought and fuller life we lose who do not keep our minds open to the soothing benison of the messages which Nature is bringing to us from the Father of all.

The gains of the movement can hardly well be reckoned up. It was a popular movement. This may mean what is good or what is bad. In a good sense it was popular, for it recognized more than did the poets of preceding ages the ministry which poetry might exercise towards the people. It was a movement against conventionality and towards naturalness. This virtue it shared with almost every other new movement in art, letters and faith; for is it not often the case that a new movement is the swing of the pendulum away from the art or religion which have tended to become conventional? But though it shared this feature with many other movements, we must not on that account be less grateful to those who opened new avenues when men were seeking wider room in which their developing powers might exert themselves.

It was an opportune movement, for it came at the time when science and the mechanical arts were about to lengthen their cords and strengthen their stakes. It was well that at such a time, when science might have monopolised the human mind and the increased opportunities of wealth might have engrossed men's thoughts, singers and writers should have arisen who conjured up before the popular imagination another world, ideal if you will, but not less real on that account, which might woo them from vulgar and too earthly thoughts. We cannot estimate how much the reading of the poetry and romances of Walter Scott has kept alive in the popular mind an interest in and a love of things better than those which perish in the using.

Such gain the movement gave to English life. How much the national life was enriched we can perhaps estimate by asking ourselves whether we could afford to lose Shelley's "Skylark," "The Ancient Mariner," "The Ode to Immortality," "The Lay of the Last Minstrel," not to mention a host of other now familiar and cherished works. The movement bequeathed us rich legacies. Rich as these were, we must leave them behind: for we are called to press onward and ever up the steep of the hill on whose summit the eternal sunlight shines. We in our generation have our work to accomplish, as these men had; but the songs which these men sang will cheer us as we struggle upwards. We shall share the feelings of Wordsworth when, on the side of the hill, he heard the song of the solitary reaper rising from the vale below:—

"Whate'er the theme, the maiden sang
As if her song could have no ending;
I saw her singing at her work,
And o'er the sickle bending;
I listened motionless and still;
And as I mounted up the hill,
The music in my heart I bore,
Long after it was heard no more."

Thus it is that the songs of the past which have once struck into the hearts of men, not only find a resting-place there, but bring their inspiration to the makers of other songs.

But the critic bids us pause. He asks us whether we are sure of inspiration in the future, as in the past. We cannot prophesy; but we may at least draw our hopes from the principles which are attested by the past. And if the question is asked whether the poetry of the future is likely to be as good as that of the past, I think we may say that there is no reason why it should not. But, while we record our hope, it is only fair to say that there is no lack of gloomy-minded critics who shake their heads over all who dare to hope. In their judgment the world of letters is always on the decline.

Such folk will probably remind us of the opinion of Macaulay, which was that as civilisation advances poetry will decline. Though some would not accept so depressing a statement of the question, they might argue that with advancing knowledge the realm of imagination is necessarily narrowed, and to that degree the kingdom of poetry diminished. Science encroaches on the domain of fancy; and poesy, like a bird no longer free to stretch her wings, grows silent in her cage. The captive muse disdains to use her harp.

But is this picture an adequate or true description of the matter? Is it a fact that the advance of science narrows the domain of imagination? The analogy of the argument is faulty. It presupposes a limited territory shared between the two powers, science and song. If this is a correct picture, it follows of course that the larger the territory annexed by science the smaller is the area left to poetry. But the true picture is of another sort. Both science and poetry are as travellers exploring together the great infinite which surrounds them. Year by year science adds new territory to the domain of things known and so extends its circling border into the realm of the great unknown; but the wider her borders grow, the longer is the frontier line which separates the known from the unknown, and the greater is the realm which imagination may claim as her heritage. Or, to put it in another way, as knowledge increases our sense of the infinite grows. In the days of men's ignorance their minds did not travel far. Imagination did not penetrate deeply into the gloomy wood which surrounded her dwelling. But as knowledge extends her borders the world is found to be greater: the infinite means far more to us than it did to our forefathers. We are as those who have climbed higher, and the prospect is wider and the field of mystery which spreads beyond is greater and more marvellous than ever. In short, as I contemplate and rejoice in the conquests of science, the picture which I see grows larger and nobler. I seem to see science unfolding the gates of knowledge and saying to the children of fancy, "I have swept the wide spaces where the stars are shining, and I have seen their colour and the fashion of their being; I have weighed them in the balance, and I have marked their birth and their decay. I have pierced into the fabric of the universe and I have flung the elements of worlds upon my spectroscope. I have broken down the prison walls of envious and imprisoning materialism. I have seen everywhere life and movement. I have heard everywhere the whirring of God's chariot wheels, and I have found that the impulse of His will vibrates through untravelled and eternal regions. You need fear no more; go forth and sing freely of the living, energizing, inspiring, force divine which has worked and is working now and has worked in all the myriad worlds and interstellar areas since the first day when the stars first sang together and all the sons of God shouted for joy."

We have not yet seen among us the poet who can digest the vast knowledge of to-day and sing once more the great eternal and universal song for which human kind is ever waiting; but we have numberless poets who have seen the travail of man, the glory of God, and the wonder of life, and who have sung to us right worthily of the pain of earth and the call of heaven, and of the faith which lives imperishable in the heart of man, whose song, among the ruins of broken hopes and shattered realms, is still the song of nobler hope.

"In joy, in joy of the light to be,
O Father of lights, unvarying and true,
Let us build the Palace of Life anew;
Let us build for the years we shall not see."

Yes, the song of the true poet always ends with the "Sursum Corda" of changeless trust. We put aside the pessimism, which whispers that the bright realms in which poetry loves to ramble have passed into alien hands. It is not true that the territory of imagination shrinks as the frontiers of science are advanced. It is true that knowledge needs to be assimilated before imagination musing on new truth can take fire and break forth with unfettered voice. It is true that an age of eager acquisitiveness is not always an age of creativeness; time is needed before food can be converted into strength. But nevertheless as man's knowledge grows, the sense of our ignorance will grow, and the tireless curiosity of our nature will yearn to explore the untrodden regions. With such stimulus men will find resources out of which the art of expression will be enriched. New stops will be added to the organ, and richer melodies will roll up to heaven. If God has new light to break forth for men, be assured of this that men's hearts and imaginations will respond to the broadening light. Science, which is the art of knowing, is bound to grow; as it grows it will increase its wide spreading boughs, and offer welcome shelter to the winged ones whose haunts are between earth and sky. However great may be the tree of knowledge there will always be birds of heaven to sing among the branches.

[W. B. C.]

GENERAL MONTHLY MEETING,

Monday, April 8, 1918.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Dr. H. W. Beedham,
H. C. Jenkins,
Miss E. Noel,

were elected Members of the Royal Institution.

The Right Hon. Lord Rayleigh, O.M. D.C.L. LL.D. F.R.S., was nominated for Election as Honorary Professor of Natural Philosophy at the next General Meeting on May 6, 1918.

Professor Sir J. J. Thomson, O.M. LL.D. Pres.R.S., was nominated for Election as Professor of Natural Philosophy at the next General Meeting on May 6, 1918.

The Managers reported, That at their Meeting held this day, they Resolved, in conformity with the Bye-Laws (Article 1), Chap. XI., to issue a Notice to all the Members that at the General Monthly Meeting to be held on May 6, 1918, a Resolution will be submitted that the Treasurer of the Royal Institution, Sir James Crichton-Browne, F.R.S., be authorised to execute and sign a Power of Attorney in a Form submitted by the Dutch Government, to enable the sum of 24,000 guilders (nominal value) inscribed on the Ledgers of the Public Debt of the Netherlands in Amsterdam, to be sold with a view of the proceeds being remitted to the Bankers of the Royal Institution, to be reinvested in Trustee Securities in accordance with Article 5, Chapter II. of the Bye-Laws.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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American Physical Society—Review, Vol. XI. No. 2.
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Canada, Department of Mines—Summary Report for 1917.
Carnegie Institute—Carnegie Scholarship Memoirs, Vol. VIII. 8vo. 1917.
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Cow, Mrs. Douglas—Cinderella. By Marion Roalfe Cox.
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Concrete for March 1918. 8vo.
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Electrical Times for March 1918. 4to.
Electricity for March 1918. 8vo.
General Electric Review for March 1918. 8vo.
Horological Journal for March 1918. 8vo.
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Junior Mechanics for March 1918. 8vo.
Law Journal for March 1918. 8vo.
London University Gazette for March 1918. 4to.
Marine Magazine for March 1918.
Model Engineer for March 1918. 8vo.
Nature for March 1918. 4to.
New Church Magazine for March 1918. 8vo.
Notes and Queries for March 1918.
Power-User for March 1918. 8vo.
Science Abstracts for Feb. 1918. 8vo.
Zoophilist for March 1918. 8vo.
Electrical Engineers, Institution of—Journal, March 1918. 8vo.
Franklin Institute—Journal, Feb. 1918. 8vo.
Geographical Society, Royal—Journal, Vol. LI. No. 3, March 1918.
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Southern Slav Bulletin—Vol. XXXVI. Feb. 1918.
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Zoological Society of London—Proceedings. 8vo. 1917.

WEEKLY EVENING MEETING,

Friday, April 12, 1918.

Dr. Henry E. Armstrong, LL.D. F.R.S., Vice President, in the Chair.

Professor E. C. C. Baly, F.R.S.

Absorption and Phosphorescence.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, April 19, 1918.

General E. H. Hills, C.M.G. R.E. D.Sc. F.R.S., Secretary and Vice-President, in the Chair.

Major G. I. Taylor.

The Use of Soap Films in Engineering.

[Abstract.]

The similarity, first noticed by Prof. Prandtl, between the equations representing the strains in a twisted bar of any section and the deformation of a membrane stretched uniformly over a plane boundary of the same shape as the cross-section of the bar, and deformed by the action of a uniform pressure, has been used to find the strains in twisted bars in cases where they cannot be calculated. The uniformly stretched membrane used in the experiments was a soap film, and films were projected on the screen in such a way as to illustrate, by means of Prandtl's analogy, the principle features of the strains in twisted bars.

In this way it was shown that the strain at an internal corner becomes infinite, while that at an external corner vanishes.

As an example of the usefulness of the method in solving practical problems in engineering, the amount by which it is necessary to round off the internal corner of an L-shaped beam in order that the strain may not become too great when the beam is twisted, was worked out by means of soap films.

Apparatus designed for measuring the soap films was shown.

Some of the results obtained from the measurement of soap films are not easy to understand when applied to twisted bars. Some gelatine models were exhibited and projected on the screen which showed how the strains arise in these cases.

[G. I. T.]

WEEKLY EVENING MEETING,

Friday, April 26, 1918.

THE HON. RICHARD CLERE PARSONS, M.Inst.C.E.,
Vice-President, in the Chair.

SIR A. DANIEL HALL, K.C.B. F.R.S., Permanent Secretary,
Board of Agriculture.

Food Production and English Land.

THE war has given us a new anxiety about our food supplies and a new interest in British agriculture as the only source of food if the work of the enemy submarines, a form of blockade that had never been contemplated, attains its aim even partially.

To understand our position with regard to food production to-day we must go back to the history of British agriculture for at least forty years. Half a century ago British agriculture was easily pre-eminent, whether as regards its production from the soil, the excellence of its live-stock, or the organisation which was put into the management of the land. Then in the late seventies the great simultaneous development of the new lands of America and of ocean shipping coincided with a run of bad seasons at home and a steady appreciation of the gold standard. From all these causes combined prices of agricultural produce fell, and continued to fall all through the eighties and well into the nineties, until by 1894–5, the lowest point, farmers were ruined wholesale. If they survived they saw their capital reduced to the lowest ebb; landowners found their income from land down to the vanishing point, and could neither sell nor let, for all confidence in farming as a business had disappeared. From 1895 prices began to turn and continued to improve up to the date of the war, for a year or two before which farming had become a reasonably prosperous industry, and land was again in demand, though new capital was still to seek, so thoroughly had public confidence in agriculture been destroyed by the course of events within everyone's memory. Farming had revived, but viewed at large it was a system that attained only a low average of production, low utilisation of capital and low rents. As there were plenty of rich men in the country land was in demand for its amenities, but not as an income-producing investment.

What had happened during that period? How had men readjusted their business to the new conditions? Some by altering the character of their production, by growing materials that escaped from the full brunt of foreign competition, milk instead of corn; but in the main men had reduced their expenditure, cheapened their methods, and above all economised in labour by laying down to grass all land that was costly in labour or speculative in yield. Rents had, of course, been reduced, but men had to make a saving that was far greater than all the rent; the labour bill was the big item of expenditure which could be cut at, and grass land needs but a quarter or even a tenth of the labour that is used on the arable. A few men brought science and skill to bear and cheapened their production by intensifying it, but such men were rare because they could find in other industries a better market for their enterprise; for the majority Lawes' dictum holds, "High farming is no remedy for low prices." The chart (Fig. 1) shows the trend of events during these fateful years—here is the line showing the cause—the cash returns from the corn and meat produced from an average acre of arable land, declining from 8*l.* 10*s.* in 1875 to 5*l.* in 1895. Here is the line showing the area of arable land in England and Wales, declining from a maximum of 14 million acres in 1872 to 10½ millions in 1913. Here are the number of men engaged in agriculture, 1,270,000 in 1871, 950,000 in 1901, just over 1 million in 1911. Lastly, here is the wages curve; in spite of the diminishing returns from the industry the competition of the towns for workers had caused the average weekly rate of cash wages to rise from 12*s.* 6*d.* in 1870 to 15*s.* in 1900, and over 16*s.* in 1913. Here (Fig. 2) are the live-

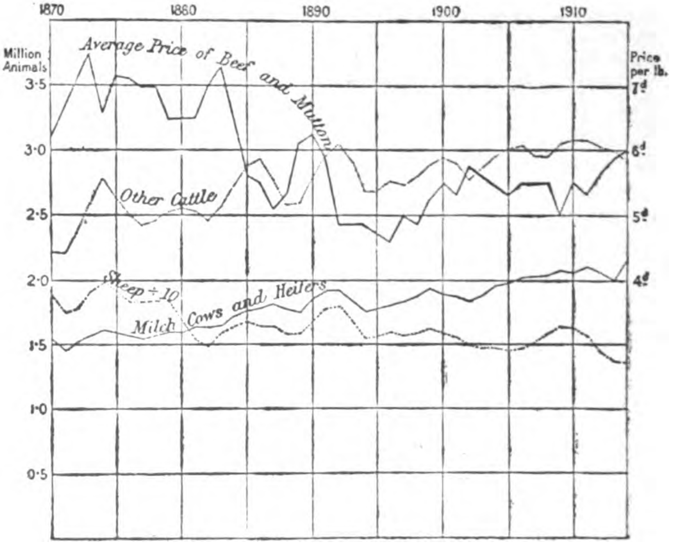

FIG. 1.

stock curves for England; sheep have declined with the arable land, for in England sheep are largely fed on the arable; cattle have increased; above all dairy stock have grown in number. In Scotland and Ireland no such great change had taken place, though the arable land has declined, and in Scotland the proportion of temporary grass reckoned as arable land has increased, yet these countries being at the outset more concerned with stock than corn have maintained their position.

How had the production of food been affected?

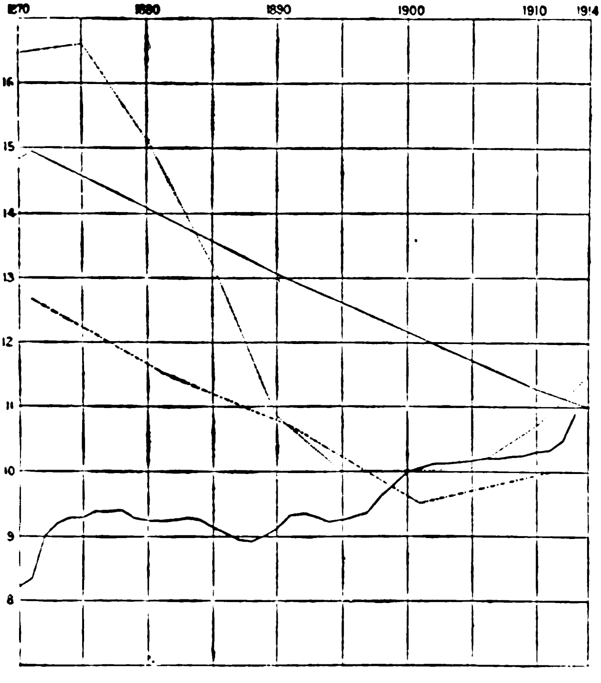

FIG. 2.

At first sight the change seems all right: corn had been exchanged for beef and milk; the nation got the richer food and left to other nations the output of cheap raw materials like wheat; the farmer was better off by concentrating on the more profitable market.

This is plausible enough until you come to consider quantities, then an immense falling off in gross output becomes apparent.

In the first place any change to animal products from vegetable means waste when the consumer is down near the line of bare subsistence. The animal has to live on ready-grown food and is a bad converter; the pig which is the best habitually eats six or seven pounds of barley meal for every pound of pork he furnishes, and fat cattle consume at least twenty pounds of essential food for each pound of beef they lay on. Of course much of this food was such as human beings do not eat, grass or roots, but some was always potential human food, and the land which grew the cattle food might have grown human food. And this brings us to the second point: the absolute production from grass land is much below that from arable land, perhaps only a third on the average if both are set to produce meal or milk, less than a twentieth if the grass produces, as it only can, animal products, while the arable is turning out human food of a vegetable nature. Consider an acre of ordinary grass land; it will produce in a year 150 lbs. of mutton, or perhaps 200 gallons of milk, but if planted with potatoes the excess of crop over seed may be expected to be 5 tons at least. Moreover, with a little bustling from manures and cultivation the 5 tons of potatoes can be made into 10; but it would be far more difficult to double the production of the grass. To take another example more in the line of general practice, land under a rotation will produce 1000 lb. of corn per acre, and out of the straw and the roots and clover that occupy part of the land will grow the same amount of meat as would have been produced from the whole land had it been under grass. So the laying down of land to grass that went on during the years 1872–1915 did not mean the exchange of corn for an equivalent of meat and milk, but a dead loss of production of food. The loss is somewhat disguised in the chart you have seen illustrating the changes in the numbers of cattle, because that took no account of the increasing dependence even of our live-stock upon imported cattle foods. At the end of the period, for the five years 1909–13, the imported cattle foods consumed in the United Kingdom amounted to over 6 million tons per annum—barley, oats, rice, cakes, molasses, etc. (see Table I.). It was all concentrated food, and translated into meat in the ratio of 7 to 1 it represented 900,000 tons of meat, or one-third of the whole amount of meat consumed in the country. Considered more nicely on its energy value it was equivalent to, and if properly used would have produced, 29 per cent. of the whole of the meat, milk, eggs, poultry and dairy produce grown in the United Kingdom. So even the grass land did not make all the animal food we counted as the result of the change in our farming; one quarter at least was the outcome of imported feeding stuffs.

And in what sort of a position had these changes—less arable land, less production, more population—landed the country at the outbreak of war?

Table II. shows the Royal Society's Committee's estimate of the amounts and origin of the food consumed by our population for the years 1909–13. We were producing in all 42 per cent. of the food

TABLE I.—CONSUMPTION OF FODDER IN THE UNITED KINGDOM, 1909-13 (THOUSAND TONS).

—	Gross Weight	Dry Weight	
		Grown in U.K.	Imported
For Production:—			
Wheat offals	2,500	440	1,760
Maize	1,700	—	1,500
Oats	3,500	2,250	750
Barley	980	420	420
Beans, etc.	500	300	150
Brewers' grains, etc.	450	240	160
Oil cakes	1,200	—	1,100
Other foods	450	—	380
Roots, etc.	44,500	5,200	—
Grass and hay	15,000	13,000	—
		21,850	6,220
For Maintenance:—			
Grass (as hay)	45,000	38,000	—
Straw	5,400	4,700	—
	50,400	42,700	—

72 per cent. of productive fodder of U.K. origin.
91·5 per cent. of total fodder of U.K. origin.

TABLE II.—CONSUMPTION OF HUMAN FOOD IN THE UNITED KINGDOM, 1909-1913.

—	Metric Tons Thousands			Energy per cent. of Total	
	Total	Home	Imports	Home	Imports
Cereals	4,865	1,010	3,855	7·2	27·4
Meat	2,685	1,615	1,070	10·5	7·0
Poultry, eggs, etc.	331	170	161	0·4	0·4
Fish	848	715	133	0·8	0·2
Dairy produce	5,232	4,704	528	9·2	7·0
Fruit	1,271	341	930	0·3	1·8
Potatoes and vegetables	5,482	4,788	694	8·0	1·5
Sugar, etc.	1,657	—	1,657	—	13·0
Cottage, farm and garden produce	3,330	3,330	—	5·2	—
				41·6	58·3

we consumed. Of meat we produced 60 per cent. of our consumption; both of wheat and bread, which represents 30 per cent. of the food of the people, we only grew one-fifth of what we consumed. What a position for a nation at war with an adversary that can make a bid to cut off our supplies! And Germany is making that bid in spite of what we held was our overmastering fleet.

Let us think for a minute of what our neglect of agriculture, our contentment with the position of being importers of half our food is costing us. First of all, we may be cut off from our food, may have to stop fighting because we have not enough to eat. It has only become perceptible in this last year that the food supply in the whole world is limited; there is not an inexhaustible reserve for us to draw upon, and external supplies are running short. Even if we escape that last despair we have to use a large proportion of the strength of our navy, strength that is wanted for fighting purposes, to protect the entry of food ships. Again, we have to pay for the food in foreign countries, pay at enormously enhanced rates at a time when we want to retain every penny within our own islands. Our food bill forms the most onerous of debts to a nation at war, debt to a foreign creditor. Food, too, must be the first charge on our shipping, and shipping is the limiting factor in this war. Suppose the shipping we must fill with our food were now free to bring American soldiers and their food to our help in France!

What could we do to meet the situation? The answer is plain—plough up our grass land and get back to the state of 1872, or even a more productive condition. But here is seen the full measure of the danger into which we have fallen; when land is put down to grass the land is still there, but before long the means of ploughing it up are gone—the men have left for other occupations, the horses are gone, ploughs, harness, buildings are lacking, sometimes even the knowledge of arable cultivation has been forgotten. If you let a building go to ruin it will not be ready for occupation at an hour's notice, still less will an organisation, a society, begin to function again.

Still, what has been done can be done. But for the first two years and more of the war it was not thought worth while to make any determined attempt to reconstruct agriculture and grow more food at home, so great was the sway of the old pre-war conception that agriculture was done for in this country, and that it was useless, even wrong, to try to grow our own food. But, late as it was, in January of last year the campaign for increased food production was set on foot under Mr. Prothero with Sir Arthur Lee as his lieutenant in command of the fighting forces. But you cannot step twice into the same river; as agriculture depends upon men, and the men available in 1917 were fatally less than those still free in 1914, the power to effect a wholesale change was gone. Still the achievement of the Food Production Department has been enormous. This year we are going to have from three-quarters to a million more acres of wheat in England and Wales than the 2½ million we had last year, and more than two million acres of permanent grass land have been ploughed up for this year's cropping, a recovery of two-thirds of the arable land that has been lost since 1872. To this we must add that a good deal of the temporary grass which occupies part of the arable land has this year been replaced by food crops, so that with the additions that Scotland and Ireland have been able to make there will probably be as much land under food crops this year in the United Kingdom as there was in the seventies. You will understand I am speaking approximately and on estimates, for the exact returns have not yet been collected. It is a great performance; if I say that twice as much has been done as Lord Milner's Committee sitting in 1914 thought was *possible* within the period, and that estimate was regarded as over sanguine, I perhaps give you an idea of the magnitude of the task.

But we are here to think of the future as well as of the present. I have said enough I hope to convince you that Britain must have a strong agriculture, must use her land to produce food, if she is to be safe as a nation in future. How are we to ensure that? I do not contemplate that we should become self-supporting in the matter of food. I do not even wish us to grow all our own wheat. When war is over we shall revert to our old standards of profit, and men will produce meat or milk, asparagus or fruit, as pays them best. But we must have more arable land, another six or eight million acres, and we must have more men engaged upon the land, so that when the crisis comes, as it may come again, we can throw all our acres into the production of essential food, enough to support us through the years of trial. With this easily possible amount of arable land we could be self-supporting for several years if we ate up our stocks of cattle, sheep and pigs, and used our land to an ever-increasing extent for the production of human food only. Our policy then must be directed towards more arable land, more men engaged in cultivation, greater responsibility for the user of land.

We have taken the first steps in that direction by the Corn Production Bill of last year. We cannot hope that men will adventure on the risk of arable farming unless they are free from the danger of the disaster that overtook the last generation of farmers. Hence the guaranteed prices of the Corn Production Bill; the State will pay a bonus on the production of wheat and oats if the world prices fall below a certain level. It is an insurance that the State deliberately enters into in order to make sure that the arable land is maintained.

Nor will men stop on the land unless their living is guaranteed. Before the war the agricultural labourer only stopped upon the land because he was too depressed to look for any better paid employment; as far as they were capable the younger ones were not staying. The Corn Production Act entails a minimum wage. Mr. Prothero was bold enough to fix it at 25*s.* a week; for the present the County Wages Committees are beginning with 30*s.* Lastly, the Corn Production Act takes power to ensure the proper user of the land; if a tenant will not farm properly, or a landlord prefers game to corn, he can be turned out and someone put in who will employ the land in the national interest as well as his own. Far-reaching principles these, revolutionary in their economics; but have they any sound financial basis? The land has after all to pay either for arable cultivation or minimum wages; can the land pay for these ideals? Time is too limited to go far into that question. I will only lay before you one consideration. The agricultural labourer before the war, as Mr. Acland has pointed out, did actually produce more stuff net out of which he and his master had to be paid than did the industrial worker, the cotton-spinner, or the steel-maker. His output was worth about 100*l.* a year net, out of which his wages had to be paid. The average farmer in England was in fact living upon the earnings of five or six men; he wanted a big return per man because he employed so few. Here, then, are the openings for economy in the new order: better organisation so that the man has not to carry so big a fraction as a master; better efficiency so that the output of the man is greater.

Time does not permit of the consideration of these latter points, organisation and efficiency. Here we should all agree that education and research have their part to play in the reconstruction, as great, if not greater, than the fiscal and legislative matters of which I have been speaking. We began with considering that the nation must grow food in order to survive as a nation: we end with the conception that the nation must grow men with brains if it is to have food.

[A. D. H.]

ANNUAL MEETING,

Wednesday, May 1, 1918.

SIR JAMES CRICHTON-BROWNE, M.D. F.R.S., Treasurer and Vice-President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1917, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Twenty-three new Members were elected in 1917.

Sixty-two Lectures and Nineteen Friday Discourses were delivered in 1917.

The Books and Pamphlets presented in 1917 amounted to 299 volumes, making 513 volumes (including Periodicals bound) purchased by the Managers, a total of 812 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committee of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following gentlemen were unanimously elected as Officers for the ensuing year :—

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.

TREASURER—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.

SECRETARY—Colonel E. H. Hills, C.M.G. R.E. D.Sc. F.R.S.

MANAGERS.

Henry A. Armstrong, LL.D. F.R.S.
Sir William Phipson Beale, Bart., K.C. M.P.
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Donald W. C. Hood, C.V.O. M.D.
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The Rt. Hon. Clere Parsons, M.A. M.Inst.C.E.
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J. Emerson Reynolds, M.D. F.R.S.
The Rt. Hon. Lord Rothschild, F.R.S.
The Rt. Hon. Lord Wrenbury, P.C. M.A.

VISITORS.

K. A. Wolfe Barry, M.Inst.C.E.
Sir Wm. H. Bennett, K.C.V.O. F.R.C.S.
John G. Bristow, M.A.
John F. Deacon, M.A.
Edward Dent, M.A.
Sir James J. Dobbie, LL.D. F.R.S.
John A. W. Dollar, F.R.S.E.
W. B. Gibbs, F.R.A.S.
Wm. J. Gow, M.D. F.R.C.P.
W. A. T. Hallowes, M.A.
Sir Alexander Pedler, C.I.E. F.R.S.
Hugh Munro Ross, B.A.
Joseph Shaw, K.C.
John Strain, M.Inst.C.E.
Sir Henry Wood

WEEKLY EVENING MEETING,

Friday, May 3, 1918.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

SIR GEORGE GREENHILL.

The Spinning-Top in Harness.

"To those who study the progress of exact science, the common spinning-top is a symbol of the labours and perplexities of men who had threaded successfully the mazes of planetary motion. [A sly dig at Newton, I fear, in his struggle with Precession.] The mathematician of the last century [the eighteenth], searching through Nature for problems worthy of his analysis, found in the toy of youth ample occupation for the highest mathematical powers."

These are Maxwell's words, reprinted in his Collected Works, I. p. 248, from his memoir on "A Dynamical Top," and here is the top, designed to show off the results of his theory. The toy of youth is shown in a collection here on the table. But to make the motion visible to this audience, we have chosen some on a larger scale, as this 52-inch wheel of Mr. C. V. Boys's Otto bi-di-cycle. The point of the stalk is placed in a small cup, and the wheel is spun by hand. Dynamical similitude, as well as geometrical, is obtained by making the rim speed proportional to the square root of the diameter; so that here a slow rotation, about one revolution a second, will be enough, contrasted with the 20,000 revolutions a minute in a gyro-compass, or 2,500 per second of the rifle bullet—that is, 150,000 per minute.

The ordinary toy is too small to be seen at a distance, and a string must be used to spin it, to give velocity enough. A glance at the length of his string will show us when the operator will bungle—foozle the spin. This will be apt to happen if the string is longer than this, half the stretch of the arms—a fathom. Here's the cheap-jack's secret, selling this toy gyroscope here.

The attention to reach is the great secret in boxing. The master buttons on the gloves of the novice, and standing up with hands behind his back he invites attack :—"Hit me !—as hard as you can. Kill me, if it pleases you ; and it does'nt hurt me !" How mortifying to find that he is always just out of reach, drawing his head back quite a short way!

The master teases her by quoting the advice in his Latin grammar, how to fight the girl as he was taught as a boy—"Rixa, si rixa est, ubi tu pulsas, ego vapulo tantum" ("A fight where I hold out my head, for you to whack it.")

Grasp the stalk, and brandish it, with the wheel at rest. No gyroscopic effect is felt: the wheel is inert. But give it a spin first, gently at a start with the novice, as the result will take him by surprise when the stalk is brandished. It flies to one side or the other.

I avail myself here of the privilege of addressing a learned audience, and so employ some of the technical jargon of our science. Represent the angular velocity and momentum of the spinning-wheel by a right-handed screw vector, as also the axis of an applied couple. By a vector that is right-handed we mean one such that the direction of the vector is the advance with a right-handed screw propeller.

Then if the wheel is swung $\left\{\begin{matrix}\text{upward}\\ \text{downward}\end{matrix}\right\}$ an angular momentum is communicated about an axis drawn to the $\left\{\begin{matrix}\text{right}\\ \text{left}\end{matrix}\right\}$, and the axle swerves the same way $\left\{\begin{matrix}\text{right}\\ \text{left}\end{matrix}\right\}$ unless prevented by a couple given by the hands, represented by an axis drawn $\left\{\begin{matrix}\text{upward}\\ \text{downward}\end{matrix}\right\}$. Swung to the $\left\{\begin{matrix}\text{right}\\ \text{left}\end{matrix}\right\}$, angular momentum is communicated about a $\left\{\begin{matrix}\text{downward}\\ \text{upward}\end{matrix}\right\}$ axis, and the axle swerves $\left\{\begin{matrix}\text{down}\\ \text{up}\end{matrix}\right\}$, and requires to be held $\left\{\begin{matrix}\text{up}\\ \text{down}\end{matrix}\right\}$ by a couple with axis to the $\left\{\begin{matrix}\text{right}\\ \text{left}\end{matrix}\right\}$.

With two screws on a ship or flying-machine they are generally spun opposite ways, so one of them will be left-handed. On a left-handed screw these directions are reversed, and we should get into confusion. Better, then, keep to the right-handed screw of ordinary practice, in any popular explanation. These are influences felt in a flying-machine, especially if provided with a Gnome revolving motor — a spinning-top in harness. The name Gnome seems to have been anticipated by Pope, in his lines

> "Know, then, unnumbered Gnomes around thee fly,
> The light militia of the lower sky,"

in the poem of the "Rape of the Lock."

Putting the helm (joy-stick the airman calls it) of the flying machine to port, as a sailor would say, a couple is called up from the rudder tending to turn the head to starboard, and the couple axis is downward, and the nose of the machine dips. To ascend, the helm must be put to starboard. But to turn the machine to $\left\{\begin{matrix}\text{starboard}\\ \text{port}\end{matrix}\right\}$ by utilizing the gyroscopic domination, a horizontal rudder must be used, and its helm put $\left\{\begin{matrix}\text{down}\\ \text{up}\end{matrix}\right\}$ vertically.

Thus the top in harness is like the Irish pig, in Professor Perry's simile, who cannot be persuaded to accompany his master to market except by steering him sideways into the ditch, a veritable *tour-de-force*—"transferring forces round the corner," in the title of the pamphlet on the Sperry Gyro-Compass. This sidelong force makes its appearance in the electrodynamical equations, and is called the gyroscopic term in consequence.

The wooden ball on this wheel was fitted originally as a protection for the head underneath when the wheel was suspended from the ceiling. The ball comes in useful here as showing the influence of the rounding of the point of a top in enabling the top to rise, when free to wander about the floor, and not constrained in this small cup. It is the friction of the ball-point causes the top to rise, but ultimately kills out the rotation.

We try to avoid in these experiments the hideous unreality of perfectly rough and perfectly smooth, in the school-book jargon, by spinning the top either in the small cup or else on the rounded point on the floor. The wheel comes to rest when the rim reaches the floor, and to study the motion when the axle makes a greater angle with the zenith, and moves about in the neighbourhood of the nadir, we turn to this other apparatus, an ordinary 28-inch bicycle wheel—the axle screwed into a steel stalk—a short length of rifle barrel, suspended from a lug on a bicycle hub, allowing motion in altitude and azimuth. An ordinary hook attachment would serve as well, I believe, if well oiled. The hub is fastened in this iron bracket, bolted to the underside of a beam or sleeper, and this should be heavy enough to absorb vibration—not a thin board, or lath, or little stick as I found them trying in Rome with the present I had made of the apparatus. The model is easily constructed, as the complicated parts, wheel and hub, can be bought cheap, ready made. Spin the wheel by hand ; and project the axle, to obtain any desired gyroscopic motion—undulating, looped, cusped.

The analytical mathematical theory is abstruse, and requires the elliptic function, in a much more complicated form than for the simple pendulum in plane oscillation of finite extent. But here are two simple cases of motion where the solution is quasi-algebraical, and so may interest the mathematical student present :—

I. Hold the axle out and project it horizontally, without spinning the wheel, when the motion is that of a spherical pendulum, a simple plummet at the end of a thread.

II. Spin the wheel, and allow the axle to fall from rest at a cusp at an upward angle so that the axle reaches the horizontal in its lowest position, rises again, and so continues.

The exact interpretation is rather delicate of the rotation of the wheel about a moving axle. Thus, although we start with the wheel at rest on the axle, we cannot turn it by twirling the axle. But moving the axle round in a conical way to rest again, we find that the wheel can be turned round on the axle, to an extent proportional to the conical angle. And so we answer the challenge of Aristotle, to turn a sphere round that is perfectly smooth, or spitted on a perfectly smooth axle along a diameter. Anyone can show this off with a pencil held between the finger and thumb.

The angles employed by Euler in 1750, and denoted by θ, ψ, ϕ, are still in use in modern analysis.

- θ For altitude or nutation.
- ψ For azimuth or precession, clamping the wheel with the thumb.
- ϕ For rubbing displacement and velocity of the wheel over the axle, when unclamped, whether at rest or in movement, the axle having alt-azimuth suspension.

There is much discussion to-day of automatic gyroscopic steering control, as of a flying-machine, or a torpedo under water. Any suggestion can be tested here on these simple appliances, with ease and immediately, of harnessing the top for useful work. Thus, I hold up the wheel with the tip of a finger, and feel the pressure is the same as if the wheel was at rest. There is no gyroscopic action. But let go the axle and the gyroscopic effect is strong, especially if we attempt to hold this vertical spindle, so as to keep the wheel swinging in a vertical plane as an ordinary pendulum. A violent twisting is felt, one way and the other. The same effects can be shown off with this large wheel, in free precession when the ball-point is resting on the hand, and then gripped.

"Hurry the precession and the top rises, against gravity," is the Kelvin rule, and we see it here. It is the secret process in plate and platter-spinning. We employ the rule instinctively on the bicycle. To recover the upward position we steer into a smaller circle, and the machine rises; and this operation goes on incessantly. A bicycle cannot run straight. So also we can hurry the nutation here and examine the influence on the precession. Beating the large wheel with a stick scarcely makes it flinch.

The precession in this apparatus can be hurried still more by projecting the wheel the reverse way, and we notice how violent the motion becomes: a hint to the inventor. It is well not to show this off too soon to the active mind, as it will take no further interest in the former gentle motion, and will not rest till it has smashed something.

Aristotle discusses the psychology of the child's rattle, invented by Archytas, in gratifying the sense of hearing something smash—to come down crash on this polished floor with a top; not so large as this, though. The other invention of Archytas, of the toy flying-bird, has developed in our time into a formidable engine of war. But I can still picture Maxwell, in his drawing-room, chasing such a toy of Archytas—a New Year's gift I had made to Mrs. Maxwell—and giving us a witty lecture all the time.

Another piece of advice is, "Never grasp the wheel except at the axle," or it will fly at you. But this advice was a direct challenge to the young original researcher. I had turned my back for a moment, and heard a musical sound as if a bell had been struck. There was young Research, rubbing his head. Luckily no serious harm was done, and we gave him the consolation: "It proves you are not cracked."

Poincaré was so rash as to declare the problem of three bodies a great difficulty, much more difficult than even he imagined. Here was another direct challenge to original research, to tempt him away from all other interest; as formerly, in ancient history, the lure was the squaring of the circle or trisection of an angle.

The bicycle-wheel is detached by unscrewing the pin, as so can be used, as well as the large wheel, to imitate the ordinary spinning-top. We can also realize the experiment described in Maxwell's "Life" of the "Top on the top of a top," an allusion to some forgotten toy of 1862. The large wheel is spun, and one holds the stalk while I spin the small wheel, the same way to secure stability, and place it gently in the cup here. This gives two links of a gyrostatic chain, which we may imagine repeated in a chain or pile of wheels like a will-o'-the-wisp, or suspended from the ceiling, as in Fig. 56 in Perry's "Spinning-Tops," to serve as a mechanical model of the electro-magnetic rotary polarization of light.

Sir William Thomson gave an elaborate mathematical investigation, but this can be simplified and brought into elementary treatment by considering the chain as a uniform helical polygon rotating uniformly round the vertical, as I have explained in my Report on Gyroscopic Theory. Any discussion of a double pendulum, like a bell and clapper or a chain of links, is simplified in this manner by comparing the oscillation with a steady revolving motion, throwing a shadow moving to and fro in vibration.

Put the wheel out of balance by this rod between the spokes, and hold the axle, to make it serve as a plane pendulum, to show off oscillation of any extent, or complete revolutions, without catching in any of the surroundings. Turn a bicycle upside down on a table, and there is no need to dismount the front wheel, used as a pendulum. The muscular sense of trying to hold the axle fixed will give an idea of the reaction, as for instance in ringing a large bell. The axle can be held at any angle: here nearly vertical, like the hinge line of a door, a little out of plumb, when it is called a horizontal pendulum, useful in the measurement of slight variation in gravity, such as due to lunar or tidal influence, as in Sir George Darwin's investigations.

I am reminded of another problem to interest the mathematical students I see here. Hold the axle at the slope of the edge of this tetrahedron, equivalent to stretching the simple pendulum length threefold. The beat from V to VII o'clock is the same as the beat from I to XI, when the axle is held horizontal again.

Professor Perry has written a popular book on "The Spinning-Top," in stimulating kindergarten style, but it is doubtful if he has ever seen a top, leastways of this size. And I wonder if he has ever seen this gyroscope, although it is the present I made him many years ago.

As in skating over thin ice, the novice can progress swiftly, never stopping to look down at the black water underneath; whereas if he paused to consider the depth below, he would break through and go down; so in the theory of the top the analytical difficulties would drown the beginner, if not kept out of sight as long as possible.

The kindergarten explanation of the spinning-top is ample in answer to the beginner's question of the How and Why; but in mathematical treatment it is the How Much. That is the question.

Crabtree's "Gyroscopic Motion" goes more deeply into the mathematics of the subject; and here my "Report on Gyroscopic Theory, 1914," is intended to serve for reference on the complete theory, where no analytical difficulty is avoided, when any practical problem arises for solution.

I should like to avail myself of the privilege of addressing this learned audience, but I fear time will not allow it, to quote from my Report the statement of the exact theory in a few lemmas, given in Poinsot's style, recommended by Maxwell (I. p. 248), as a power of mathematics more searching than the Calculus, where ideas take the place of symbols, and intelligible propositions supersede equations. This would be in preference to the purely analytical treatment of Lagrange in his "Mécanique Analytique," not so much to Maxwell's taste. The translation of the lemmas into analytical equations introduces the elliptic integral in all its forms, first, second and third; and the further development could not be compressed into a lecture of one hour, so I refer you to my Report.

But I call your attention to these model deformable Henrici hyperboloids, passing through the shape of a confocal system. They were employed by Darboux for the material representation by geometrical constants of a state of top motion. Calculation was thereby replaced by measurement on a drawing.

Then there is Kirchhoff's "Kinetic Analogue," of the bent and twisted wire, to assist in making a mental picture of the top motion in all its complexity. The "Analogue" states that if an elastic round wire, rod, or shaft is bent and twisted into its most general tortuous curve under the action of an equal opposing wrench at each end, the shape of the curve is such that if a point moves along the curve with constant velocity the hodograph of its motion is a spherical curve which can be identified as the curve described by a point fixed in the axle of a symmetrical top spinning about a fixed point, as in this small cup, and in the same period by a proper choice of the constant velocity.

In most practical applications the nutation is small and imperceptible, though never absent entirely, and the motion is apparently steady, with the axle at a constant inclination, and moving round with constant precession. In the "Kinetic Analogue" the shaft is sprung slightly from the straight.

The curious property of a spinning body in rising erect in opposition to gravity, or of running along like a hoop or bicycle without falling over, has directed attention to the distinction between Balance and Stability, according as it is Statical or Dynamical.

I recall the lecture here of Lord Kelvin's, just twenty-five years ago, Isoperimetrical Problems—"Dido, or Making things spin," on a sheet of plate-glass fenced with a frame.

Since Newton compared himself to a child gathering pebbles on the shore, he set the fashion for his rivals in making them spin. But Newton took it for granted his audience knew he was quoting, against himself, the lines from "Paradise Regained":—

> "Many books
> Wise men have said are wearisome.
> Who reads incessantly, and to his reading brings not
> A spirit and a judgment equal or superior,
> (And what he brings, what needs he elsewhere seek ?)
> Uncertain and unsettled still remains,
> Deep versed in books and shallow in himself,
> Crude or intoxicate, collecting toys
> And trifles for choice matters, worth a sponge,
> As children gathering pebbles on the shore."

Demosthenes tells us he put the pebbles in his mouth he gathered on the shore, as a cure for his stammer and indistinct articulation.

In the contrast of balance, statical and dynamical, the C. G. in statical equilibrium seeks the lowest position it can find; but it rises as high as it is able in dynamical stability of balance, as of a sleeping top or bicycle. A top is said to sleep when spinning steadily upright; man or an animal sleeps lying down, with the C. G. low. But for ease of progression a man assumes the noble upright attitude of a biped, not on all-fours, or rides upright on the back of a horse or on a bicycle. Any burden, rifle or knapsack, he carries as high as possible. Mounted still higher on stilts, his progress is not more difficult, with the confidence of experience. Confusion between statical and dynamical stability of balance has led to serious mistakes and misapplication of theory, as of lowering the soldier's knapsack, or ballasting a ship too low and so making it uneasy among the waves, as recommended by Euler, a Swiss admiral, or spreading the railway gauge to lower the boiler and carriage body between the rails, in Brunel's idea. The modern locomotive is seen to-day high up over the wheels, as high as it can go under the old cramping limitation of the loading gauge of bridge and tunnel.

A literary friend in the audience has called my attention to de Quincey's account of a wonderful brother who claimed the power of rising against gravity, to walk like a fly on the ceiling, provided with spin enough, but that he would require the flagellation of a whip-top in harness, emblem of fortitude in adversity. "Tu ne cede malis, sed contra audentior ito." And here, too, we may find an explanation of the simile in the epigram of Callimachus, advice against marrying a rich wife.

Without attaining so far as the positive levity of de Quincey's brother, we have seen how a top can be made to climb a pole, in the model described to the Royal Society in their Proceedings here by Mr. Tournay Hinde. And Mr. Brennan can make it run along the tight rope or on a single rail, concealed in harness inside a carriage, to which it gives the upright stability, acting as automatically as the brain in riding a bicycle.

In the description of the American poet—

"Are you the Mr. Brennan makes gyroscopic tops
To keep a car in balance when it runs or if it stops
On single rail or wire rope that's stretched across a chasm?
Pray write and tell me, Mr. Brennan, if you're the man that has 'em."

Axial stability of motion of an elongated body through the air, an arrow, bullet, or shell, is maintained by the gyroscopic action of the spin imparted by the rifling; and the calculation of the least amount required is a delicate question of dynamics. No more spin should be given than absolutely necessary, or the shell or bullet will be uneasy in flight, as a ship is uneasy among waves if bottom-heavy, as recommended by Euler, the weights stowed too low.

Passing from small to large applications, the Parsons turbine in the steamship requires to be treated on gyroscopic theory for motion among the waves. Rolling does not affect them, but the internal stress due to pitching becomes important, and must receive investigation. So, too, if electric dynamos are mounted with axle across the ship, they are very sensitive to the rolling motion, and are heard squealing and complaining as the ship rolls.

When a vessel proved a heavy roller, a cure could be made by fitting bilge-keels, but at a permanent loss of speed in all weather, rough or smooth, of a knot or two. Schlick's sea gyroscope will cure the rolling, with no sacrifice of speed; it need not be put in action till wanted, and requires little power to keep it going. The gyroscope consists of a heavy horizontal fly-wheel, like this top, harnessed in gimbals, and controlled by a hydraulic buffer in the line of the keel. The damping action of the buffer can be regulated by a valve to suit the period of the waves, and it makes the fly-wheel react against the rolling, and kill it out. The inventor is said to have been offended when his apparatus was found more useful still in increasing the rolling, and maintaining it, in this case of an ice-breaker, to worry a way easier through the pack; or even in working off a sandbank. A different setting of the buffer valve was all that was required.

A spinning-top stands up vertical in a smooth cup, even when the cup is moved about, as on a rolling ship, and as I move the Maxwell top here; so that if the top carries a polished mirror it can serve as the mercurial horizon does on terra firma, and so give an altitude when the sea horizon is obscured. The idea was suggested by Serson in 1744, and the enterprising Admiralty of that day did not crab the idea straight off with the usual "won't work," but sent him to sea in the "Victory" to make a practical test; unfortunately the ship was lost with all hands on the Casquets near Alderney. A specimen from the King's collection is preserved in King's College, Strand. The idea has been revived of late years by French navigators, as the Fleuriais gyroscopic horizon; it is claimed to give good results, in skilful hands, where an ordinary observation would be impracticable through fog.

But the most important service to navigation in recent times of the top in harness is the gyroscopic compass. The idea was suggested by Sir William Thomson, but the high spin requisite could not be realized in his day till the great improvement arrived in modern mechanical skill of an Anschutz or Sperry, as a steel fly-wheel was required, some 4 inches in diameter, spun at 20,000 revolutions a minute. The axle, mounted freely, is always striving after the position as close as it can get to the direction of the polar axis, and so carries the compass-card with it pointing due north, with no magnetic variation requiring constant correction.

Because in modern swift steam navigation across the Atlantic, where the great circle course must be maintained, practically the only nautical observation required is for azimuth, in its correction of magnetic variation; and there is no variation in the gyro-compass. A specimen would be too complicated and delicate to show off in this room. And if any young researcher should take it into his head to test the action by pushing the card away from its course, it would take an hour or more to swing back into place again.

But the greatest spinning-top we know is this Earth itself, spinning round once a day, with the axle pointing at the Pole Star. Ancient astronomical observation reveals a precession, as in this Maxwell top, twirled with the left hand, so that the pole is making a circuit of the sky, which will be completed in 26,000 years. Since Homer's day the pole has made more than one-tenth of the way round, and the constellations have changed from one sign of the Zodiac well through the next, and back beyond. We are able, then, to assign a date to Homer and Hesiod from their astronomical allusions. Thus the nymph Calypso gives Ulysses his final instruction in navigation, how to keep off Africa, before he sets sails on his raft from Gibraltar :—

> "Never to let the Bear take a bath—
> He alone should be unsharing of the baths of Ocean,"

not setting below the horizon any star of him. To-day these instructions would land Ulysses well ashore, some 600 miles on Africa. Ulysses could take his latitude with a piece of string, one end held up to the pole, and sliding the other fingers to cover some well-known star, sweeping the hand round to see if the star would graze the horizon, in which case the polar distance of the star is equal to the latitude. Two such observations on different stars would fix his position, on Sumner's method, provided he had a chart; and Lord Kelvin amused himself on his yacht in testing the primitive methods of Ulysses and the old Greek navigator against the modern instruments of observation, sextant and chronometer, with Nautical Almanack.

The Maxwell top may represent our earth and its precession—called precession because the seasons come up to meet the sun earlier, about twenty minutes in the year, which makes the 26,000 years for all way round. In the ancient tradition there was formerly no obliquity of the ecliptic, and the year was one perpetual spring. But after the Fall of Man, in the Greek legendary astronomical theory which Milton has thrown into verse in "Paradise Lost" :—

> "Some say he bid his angels turn askance
> The poles of earth, twice ten degrees and more,
> From the sun's axle. They with labour pushed
> Oblique the centric globe."

No particular labour would be required, we see, if only the polar axis projected, as the angel could move the poles by holding his finger against the axle and letting it run up. A reverse action at the millennium will restore eternal spring.

[G. G.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1918.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Sir George Stuart Forbes,
G. H. Heard,
Lieut.-Col. C. S. Myers,

were elected Members of the Royal Institution.

The Chairman reported that His Grace the President had nominated the following as Vice-Presidents during the ensuing year:—

Henry E. Armstrong, LL.D. F.R.S.
Sir William Phipson Beale, Bart., K.C. M.P.
Sir Charles Nicholson, Bart., M.P. M.A. LL.B.
The Hon. R. C. Parsons, M.A. M.Inst.C.E.
The Right Hon. Lord Rothschild, F.R.S.
The Right Hon. Lord Wrenbury, P.C. M.A.
Sir James Crichton-Browne, M.D. F.R.S. (Treasurer).
Colonel E. H. Hills, C.M.G. R.E. F.R.S. (Secretary).

The Right Hon. Lord Rayleigh, O.M. P.C. M.A. D.C.L. LL.D. F.R.S. M.R.I., was re-elected Honorary Professor of Natural Philosophy; and Sir J. J. Thomson, O.M. M.A. LL.D. D.Sc. Pres.R.S. M.R.I., was re-elected Professor of Natural Philosophy.

Authority of the Members required according to the Bye-Laws to effect transfer of a Dutch Legacy to the Funds of the Royal Institution.

Pursuant to Article 1, Chapter 11, of the Bye-Laws of the Royal Institution,

We, the Undersigned, hereby GIVE NOTICE that at the next General Monthly Meeting of the Members of the Royal Institution of Great Britain, to be held on May 6, 1918, at 5 o'clock, a Resolution will be submitted that the Treasurer of the Royal Institution, Sir James Crichton-Browne, F.R.S., be authorised to execute and sign a Power of Attorney in a Form submitted by the Dutch Government to enable the sum of 24,000 guilders (nominal value) inscribed on the Ledgers of the Public Debt of the Netherlands in Amsterdam to be sold with a view of the proceeds being remitted to the Bankers of the Royal Institution to be reinvested in Trustee Securities in accordance with Article 5, Chapter 11, of the Bye-Laws. The sum of 24,000 guilders being the legacy bequeathed by the will of the late Dr. Louis Bleekrode (who died on the 28th March, 1915) to the Royal Institution subject to the life interest in the income of the fund of the testator's brother, Jacques Bleekrode, who died on the 10th day of April, 1917, AND NOTICE is hereby also given that if the said Resolution shall be passed the same will be brought up for Confirmation at the Members' Meeting to be held on the 3rd day of June next.

NORTHUMBERLAND.
JAMES CRICHTON-BROWNE.
DONALD W. C. HOOD.
W. P. BEALE.
HENRY E. ARMSTRONG.
ALEX. SIEMENS.
SAMUEL WEST.
C. N. NICHOLSON.
JAS. MACKENZIE DAVIDSON.
E. H. HILLS.
JAMES REID.
W. B. GIBBS.
ARTHUR N. BUTT.
MAURES HORNER.
JAMES DEWAR.

ALBEMARLE STREET,
PICCADILLY, W.1.
April 8th, 1918.

Proposed by the Hon. R. C. Parsons, seconded by Sir Wm. Phipson Beale, and unanimously RESOLVED, That Sir James Crichton-Browne, the Treasurer of the Royal Institution, be authorised to execute a Power of Attorney in accordance with the Notice read in the above terms.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Report of Progress of Agriculture in India for 1916-17.
Indian Association for the Cultivation of Science: Proceedings, Vol. III. Part 7, 1917
American Chemical Society—Journal for April 1918. 8vo.
Journal of Industrial and Engineering Chemistry for April 1918, Vol. X. No. 4.
American Geographical Society—Geographical Review, March 1918.
Asiatic Society, Royal—Journal for Jan. 1916. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXVIII. No. 4. 8vo. 1918.
Memoirs, Vol. LXII. Part 3.
Australia, The High Commissioner for—Advisory Council of Science and Industry, Bulletin No. 6, 1918.

Bankers, Institute of—Journal, Vol. XXXIX. Part 4, April 1918. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXV. April 1918. 4to.
British Astronomical Association—Journal for March 1918. 8vo.
British Red Cross Society—Joint Financial Committee, Third Annual Report to Oct. 1917.
Canada, Department of Mines—Geological Survey: Memoirs, Nos. 99 102. 8vo. 1917.
  Iron Ore Occurrences in Canada, Vols. I.-II. 8vo. 1917.
Chemical Industry, Society of—Journal, Vol. XXXVII. March-April 1918. 8vo.
Chemical Society—Journal for April. 8vo.
Commonwealth of Australia—Report of the Executive Committee to June 30, 1917.
  First and Second Reports of the Executive Committee, 1916.
County Borough of Salford—Report, 1916 17.
Department of Scientific and Industrial Research—Advisory Council Report, 1918.
Editors—Aeronautical Journal for March 1918. 8vo.
  American Physical Review for March 1918.
  Athenæum for April 1918. 4to.
  Author for April 1918. 8vo.
  Chemical News for April 1918. 4to.
  Chemist and Druggist for April 1918. 8vo.
  Church Gazette for April 1918. 8vo.
  Concrete for April 1918. 8vo.
  Dyer and Calico Printer for April 1918. 4to.
  Electrical Review for April 1918. 4to.
  Electrical Times for April 1918. 4to.
  Electricity for April 1918. 8vo.
  General Electric Review for April 1918. 8vo.
  Horological Journal for April 1918. 8vo.
  Illuminating Engineer for Feb. 1918. 8vo.
  Journal of the British Dental Association for April 1918. 8vo.
  Law Journal for April 1918. 8vo.
  London University Gazette for April 1918. 4to.
  Model Engineer for April 1918. 8vo.
  Nature for April 1918. 4to.
  Nuova Cimento for Oct. 1917. 8vo.
  Science Abstracts for March 1918. 8vo
  Wireless World for April 1918. 8vo.
  Zoophilist for April 1918. 8vo.
Electrical Engineers, Institution of—Journal for April 1918. 8vo.
Franklin Institute—Journal, Vol. CLXXXV. No. 3, March 1918. 8vo.
Geographical Society, Royal—Journal, Vol. LI. No. 4, April 1918. 8vo.
Geological Society—Quarterly Journal, April 1918. 8vo.
Institute of Chemistry of Great Britain and Ireland—Proceedings, 1918, Part 2.
Institution of Mechanical Engineers—Proceedings, Oct.-Dec. 1917.
Klein, Sydney T. (the Author)—From the Watch Tower.
London County Council—Gazette for April 1918. 4to.
Madrid, Real Academia de Ciencias—Revista, Vol. XIX. Nos. 6–9.
  Memorias, Vol. XXVII. 1917.
  Annuario, 1918.
Manchester Literary and Philosophical Society—Proceedings, Vol. LXI. Parts 2-3.
Meteorological Office—Monthly Weather Reports for Feb.-March 1918. 4to.
  Weekly Weather Reports for March-April 1918. 4to.
  Daily Readings for Dec. 1917-Feb. 1918. 4to.
  Geophysical Journal—Daily Values for Feb.-April, 1917.

Microscopical Society, Royal—Journal for March, 1918. 8vo.
Naval Observatory—Annual Report, 1917.
New York, Society for Experimental Biology—Proceedings, Vol. XV. No. 4. 8vo. January 1918.
New Zealand, High Commissioner for—Patent Office Journal, Feb. 1918, Vol. VII. No. 2. 8vo.
North of England Institute of Mining Engineers—Transactions, Parts 1-5, 1917, and 1-4, 1918.
Pennsylvania Society—The War Addresses, 1917.
Pharmaceutical Society of Great Britain—Journal for April 1918. 8vo.
Photographic Society, Royal—Journal, Vol. LVIII. No. 4, April 1918. 8vo.
Physical Society of London—Proceedings, Vol. XXX. Part 2, Feb. 1918. 8vo.
Popovic, Professor P. (the Author)—Jugoslovenska Knjizevnost.
Post Office Electrical Engineers, Institution of—Journal, April, 1918. 8vo.
Reale Accademia dei Lincei—Rendiconti, Nos. 4-5. 1918.
Rio de Janeiro, Jardini Botanico—Archivos, Vol. II. 1918.
Robertson, T. Brailsford—The Utilisation of Patents for Research.
Ross, Hugh Monro, B.A.—Japan: The Rise of a Modern Power.
Royal Engineers' Society—Journal, Vol. XXVII. No. 4. April 1918.
Royal Irish Academy—Proceedings: Section A, Parts 2-3; Section B, Part 3; Section C, Parts 5-7. 1918.
Royal Society of Arts—Journal for April 1918. 8vo.
Royal Society of London—Philosophical Transactions, Series B, Vol. CCIII.
Proceedings, A, Vol. XCIV. No. 660; B, Vol. XC. No. 626.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXIV. No. 4, April 1918. 8vo.
Smithsonian Institution—Miscellaneous Collections, Vol. LXV. Nos. 3 and 10.
U.S. National Museum, Bulletin No. 101.
South Africa, Department of Agriculture—Bulletin, No. 8, 1917. 8vo.
Statistical Society, Royal—Journal, Vol. XXXI. Part 2, March 1918.
Tôhoku Mathematical Journal—Vol. XIII. No. 2, Feb. 1918.
United States Department of Agriculture—Journal of Agricultural Research, Feb.-March 1918. 8vo.
Experiment Station Record, Vol. XXXVII. No. 9, Feb. 1918.
University of California—Collected Reprints from the George William Hooper Foundation for Medical Research, Vol. II. 1916–17.
Yorkshire Archæological Society—Journal, Part 96.

WEEKLY EVENING MEETING,

Friday, May 10, 1918.

THE HON. RICHARD CLERE PARSONS, M.Inst.C.E., Vice-President, in the Chair.

PROFESSOR F. GOWLAND HOPKINS, M.B. D.Sc. F.R.S.

The Scientific Study of Human Nutrition.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, May 17, 1918.

SIR WILLIAM PHIPSON BEALE, BART., K.C. M.P., Vice-President, in the Chair.

ALFRED BARTON RENDLE, M.A. D.Sc. F.R.S.

The Story of a Grass.

THE grasses form one of the largest and most widespread families of flowering plants; they are adapted to very different conditions of soil and climate, but have a remarkably uniform plan of structure. Wherever conditions allow of plant-life on land, there, almost without exception, the family is represented. In the aggregation of many individuals of one and the same, or a few species, either growing alone or densely scattered through a mixed herbage, covering large areas, it forms a pre-eminent type of the earth's vegetation; as, for instance, in the grass-carpets forming the meadows and pastures of temperate or cold climates, or the coarser growth of steppe or prairie vegetation. These sociable grasses are of importance in that they protect the soil from too great evaporation of water, keeping it warm and moist, and covering other plants in the resting stage during cold or dry seasons. The penetrating effect of their roots and creeping stems helps to break up a stiff soil.

The Reed-grasses invade fresh-water lakes, pools and streams, and even sea-inlets; the remarkable spread in recent years of a hybrid, *Spartina Townsendi*, on various parts of the South Coast supplies an interesting example in land-reclamation. Other species are characteristic of sand-dunes, where their long penetrating underground stems serve as sand-builders, and are hence of great economic importance. Our cereals are grasses, though owing to long cultivation their wild progenitors are often unknown or uncertain; Sugar-cane is also a grass. Bamboos are large grasses with a woody, generally much-branched stem, often attaining tree-like proportions.

The general plan of structure of a grass, as seen in one of our common species, shows a fibrous root; a stem leafy at the base, and in annual species lengthening only to bear the flowers; the nodes and internodes of the stem or culm; the sheathing-leaves arranged right and left in two rows; and the numerous flowers carried up at the end of the stem. Branches are developed from the lowest nodes; branching from the upper nodes is rare in grasses of the temperate zone, but occurs in many tropical species and is characteristic of Bamboos. If the branch-buds remain within the leaf-sheath the new shoots are forced upwards, and a tufted or tussocky habit results; this is the cause of the tillering of cereals, and is characteristic of steppe and savanna vegetation, and open places generally in the warmer parts of the earth. In other cases the buds soon break through the leaf-sheath, and then often grow horizontally as long thin stolons: turf-formation is the result of such "extra-vaginal" growth. Continued growth in length of the stem is localized at the base of the internodes. The external swelling, or "node," belongs to the sheath, at any rate so long as the internode above it is growing; its function is the raising of culms which have been "lain" by wind or otherwise.

The rigidity of the hollow culm is due to a ring of strengthening tissue just beneath the epidermis and round the conducting bundles. The culm is an excellent adaptation for the rapid production of a tough flexible stem of a temporary nature, with a minimum expenditure of material.

The character of the blade often reflects that of the environment, thin and flat in shade-grasses, and narrow or often rolled where exposed to strong sun or drying wind; the structure of the blade shows an interesting variety in this connection.

The flower and the association of the flowers, or inflorescence, supply a very characteristic feature of the family. The unit is the spikelet, which contains one or more flowers. A comparison of the grass-spike with an Iris bud or the spike of a Gladiolus indicates a common plan of arrangement, and that the characteristic features of the grass are the absence of coloured petals, and all that this absence implies, and the development of the protecting bracts or "glumes" Air-movements are the agents by which grass-pollen is transferred; the petals are reduced to small scales, the function of which is to separate the protecting glumes to allow the stamens to emerge. The beauty of the grass-flower is in many cases due to the colour of the protruding, lightly hanging anthers. Some grasses are self-pollinated, as generally wheat, and in some races of barley the flower never opens. There is great variety in the form of the grass-inflorescence, depending on the form of the individual spikelet (due partly to the number of the flowers and partly to the form of the glumes), on the presence or absence of stalks, and on the degree of branching.

The fruit is also very characteristic: it rarely opens to discharge the single seed, and is often united more or less with the glumes, which aid in its distribution. In all wild grasses certain parts of the spikelets or of the inflorescence fall off with the fruit. In the one-flowered spikelet of *Agrostis* the axis separates above the empty glume; in the several-flowered spikelet of the Fescue the spikelet itself breaks up. Where the spikelets are arranged in a close spike the axis often divides so that one spikelet falls off with each joint, as in wild barley. These arrangements are generally lacking in cultivated cereals, but present in native races so far as they are known; compare, for instance, wild and cultivated barley. The bracts or glumes are especially important as a means for distributing the fruit: being light and membranous they act as wings. Long hairs borne on the glumes or parts of the axis may act as aids to wind-carriage, as in the Reed and Sugar-cane. In other cases the awn of the glume answers the same purpose; this is especially well seen in the feathery awns of *Stipa*, which includes several steppe-grasses, where the point of the awn also forms a mechanism for fixing the seed in the ground.

[A. B. R.]

WEEKLY EVENING MEETING,

Friday, May 24, 1918.

Henry E. Armstong, LL.D. F.R.S., Vice-President, in the Chair.

Lieut.-Colonel A. G. Hadcock, K.B.E. F.R.S.

Internal Ballistics.

The object of the lecture was to draw attention to a recent view of the behaviour of modern propellants when ignited in a gun. It was pointed out that Benjamin Robins in his "New Principles of Gunnery," published in 1742, gave an indicator diagram (see Fig. 1) of the pressures developed by the powder gas along the bore of a gun. Robins assumed that the temperature of the inflamed powders was the same as that of red-hot iron, and concluded from the amount of gas produced from a unit weight of powder as ascertained by Hawksbee, that the pressure of the gas, just before the shot moved, was 1000 atmospheres, or 6·6 tons per square inch. Subsequently Dr. Hutton proved that this estimate was much too low, and that it might be doubled at least.

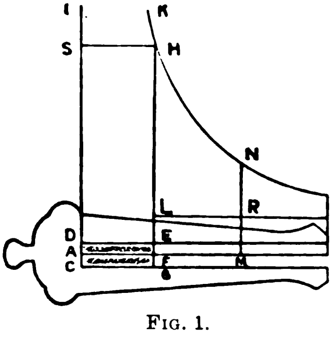

DIAGRAM FROM ROBINS' NEW PRINCIPLES OF GUNNERY, 1742

Fig. 1.

According to modern views, when the charge is ignited in a breech-loading gun a somewhat complicated series of incidents occur.

(1) The charge begins to burn at atmospheric pressure.

Most artillerists have so far treated the problem of internal ballistics either (a) as if the gun was a closed vessel for each position of the projectile, as in Centerval's and Mansel's methods, or (b) that the pressure, for each position of the projectile, bears some definite relationship to the pressure which would have been developed had the charge been burnt in a closed vessel of capacity equal to that of the chamber of the gun. This method of treatment has been adopted by Gosset, Heffernan, Charbonnier, and others.

Both these assumptions require some qualification, because in a closed vessel no linear motion is given to the charge, while in a gun a considerable amount of energy is absorbed by the charge having to be put into motion. Thus we find that the pressure on the breech of the gun is higher than that on the projectile. The pressure on the base of the shot is that due to putting the shot alone into motion, while the higher pressure on the breech is due to the additional work required to be done in putting also into motion the weight of the gas or charge. Direct experiment proves this to be the case, and usually the difference in pressure on the breech and on the base of the shot amounts to between 1 and 2 tons per square inch.

By assuming that the pressure falls uniformly from breech to the base of the shot and that the mean velocity of the gases and unconsumed part of the charge is four-tenths to one-half that of the projectile, we can prove that the following relation holds between the pressure on the breech and that on the shot—

$$\frac{\text{Pressure per square inch on breech}}{\text{Pressure per square inch on shot}} = \frac{4}{10}\frac{w}{\mathrm{W}}f + 1$$

where w weight of charge in pounds; W the weight of the shot; and f the factor of effect.

Another way of looking at this is to remember that when a column of fluid under pressure is put into motion a certain amount of the pressure is lost. The amount so lost depends on the velocity. In our case we have the gas under high pressure. Part of it is in contact with the shot and moving at the same velocity as the shot; the remainder has a gradually less linear motion, until at the breech end of the gun there is no relative motion. It follows then that the forward part of the gas, which has a considerable velocity, will have less pressure than that part in contact with the breech end of the gun.

In a gun the space behind the projectile continually increases as the projectile travels along the bore; thus each portion of the gas as generated may, for our purpose, be considered to expand according to the law $p\,(v' - a)^{\epsilon} = k$ (a constant), and do work on the shot. In doing so it would lose heat, but at the same time the charge continues to burn and additional heat is given to the already generated gases: thus our hypothetical expansion is neither adiabatic nor is it isothermal. For simplicity we suppose that no heat is imparted to the walls of the bore, but any loss from this cause is accounted for in the factor of effect.

If now we consider that some definite fraction of the charge has been converted into gas, the gas expanding according to a definite law

$$p\,(v' - a)^{\epsilon} = p\left(\frac{v - a}{z}\right)^{\epsilon} = \text{constant}:$$

as though no more of the charge would be burnt, but that the proper amount of additional heat from the unburnt part of the charge is imparted to the expanding gases—then a set of curves can be drawn somewhat similar in character to those of Fig. 13 in Clerk Maxwell's "Theory of Heat," 1897. Thus curve 1 may mean that a fraction of 1/10 of the charge has been converted into gas; curve 2, 2/10 of the charge, and so on up to 10, when the whole has been converted into gas.

These curves are lines of equal combustion, but for shortness may be called Isopyric Lines. The trace of each isopyric line shows the expansion curve of the gas then generated, on the supposition that the remainder of the gas remained unburnt.

For each isopyric line, if the position of the shot in the gun is known—that is to say, if the volume is known—then the pressure can be at once determined, or if the pressure is known, the position of the shot can be found.

If it is assumed that after a fraction z of the charge has been burnt no further combustion takes place, then the energy of the expansion of the powder gas generated from this fraction z will follow the usual adiabatic law. But since the rest of the charge continues to burn, additional heat is imparted to the already generated gases, so that the assumed expansion will not be strictly adiabatic, but will follow some law intermediate between adiabatic and isothermal expansion.

For adiabatic expansion the power to which the volume function must be raised is 1·41 for most gases, but according to actual experiments on petrol engines, a better value for these engines is 1·23, while with the high pressure used in guns the usual value has been taken at 1·2. For isothermal expansion the volume function power is of course 1. We may consequently accept the relation

$$p\left(\frac{v - x}{z}\right)^{\epsilon} = k,$$

a constant where the power lies between 1·2 and 1·0.

It has been found that the mean value 1·1 gives results closely approximating to those obtained in actual practice. The constant k depends on the resistance offered by the driving band on the shot (a) during the initial engraving and (b) during the subsequent motion along the bore of the gun.

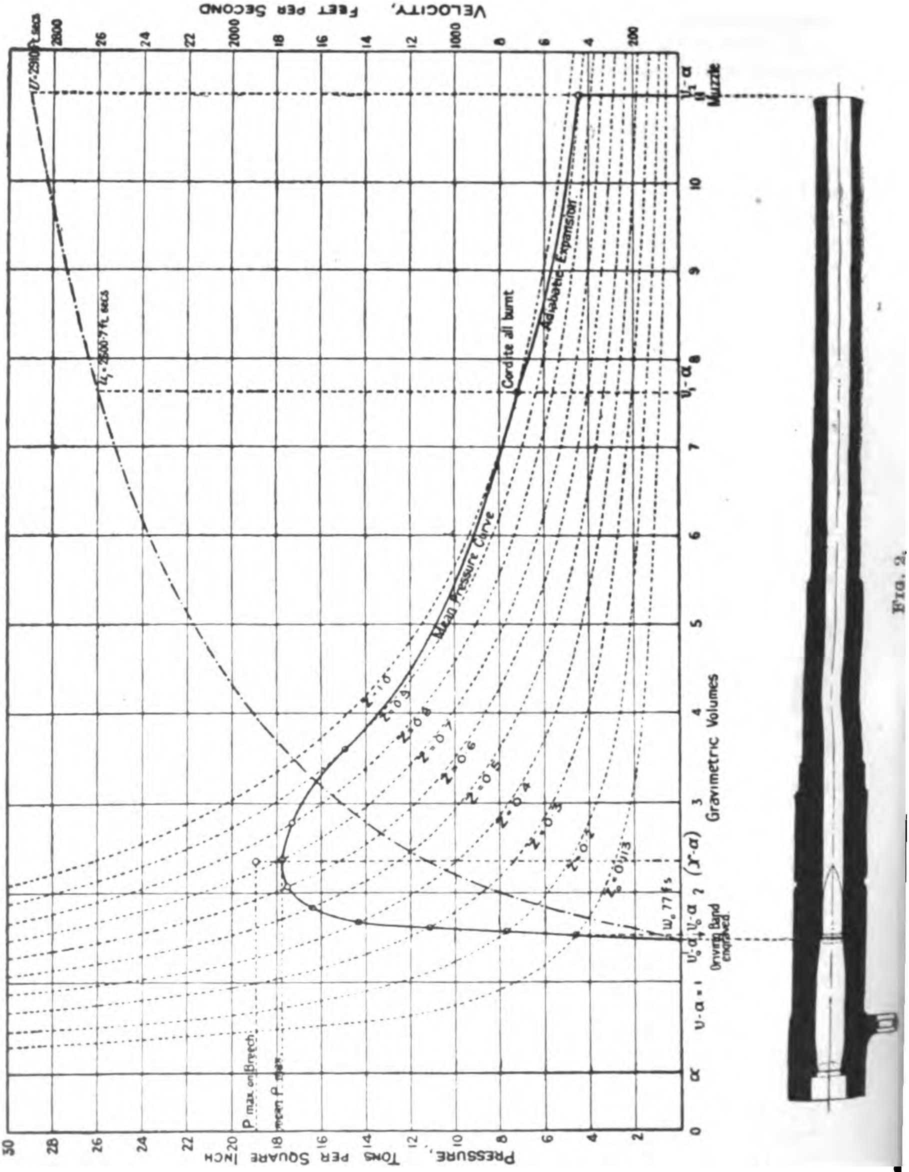
Velocity, Feet per Second
2800
26
24
22
2000
18
16
14
12
1000
8
6
4
200
Pressure, Tons per Square Inch
30
28
26
24
22
20
18
16
14
12
10
8
6
4
2
0
Gravimetric Volumes
α
v - a = 1
$V_o' - a$
$V_o - a$
2
$(r - a)$
3
4
5
6
7
$V_1 - a$
8
9
10
$V_1' - a$
Muzzle
Driving Band engraved
$U = 2910$ f. secs
$U_1 = 2500{\cdot}7$ f. secs
Cordite all burnt
Adiabatic Expansion
Mean Pressure Curve
z = 1·0
z = 0·9
z = 0·8
z = 0·7
z = 0·6
z = 0·5
z = 0·4
z = 0·3
z = 0·2
$z_o = 0{\cdot}0113$
w_o 77 f.s.
P max. on Breech
mean P max.

FIG. 2.

The gas pressure at the moment the band is fully engraved is given by the formula

$$p_0\left(\frac{v_0 - a}{z_0}\right)^\epsilon = k',$$

where k' is the constant for driving band resistance, z_0 the fraction of the charge burnt, and v_0 the specific volume of the charge when the shell has moved a distance in the gun sufficient to engrave the band.

As in the previous argument, if no more of the charge was burnt expansion of the gas would take place and further work would be done on the projectile according to the relation

$$p\,(v - a)^\epsilon = k'\,z_0^\epsilon, \text{ a constant.}$$

Thus at any expansion volume v we must always include the term $k'\,z_0^\epsilon$ as well as the usual term $k\,z^\epsilon$.

It can be easily proved that at any position up to the point where $z = 1$, that is when all the explosive is burnt, the mean pressure of the gas

$$p = \frac{k}{(v - a)^\epsilon}\,(z^\epsilon + \mathrm{K}\,z_0^\epsilon),$$

where $\mathrm{K} = \frac{k'}{k} - 1 = 0{\cdot}22121$, and $k = 66{\cdot}0$.

By combining this equation with the differential equation of motion, certain formulas are obtained which enable the motion of the shot in the gun and the pressure of the gas to be plotted as shown in Fig. 2.

[A. G. H.]

WEEKLY EVENING MEETING,

Friday, May 31, 1918.

SIR CHARLES NICHOLSON, BART., M.P. M.A. LL.B., Vice-President, in the Chair.

LAURENCE BINYON.

Poetry and Modern Life.

SHELLEY, in a letter to his wife, tells her that Byron has been reading to him at Ravenna one of the unpublished cantos of "Don Juan." He praises the "incredible ease and power" with which it is written, and he goes on to make an interesting comment. "It fulfils," he says, "in a certain degree, what I have long preached of producing—something wholly new and relative to the age, and yet surpassingly beautiful."

Byron is out of fashion to-day, and more decried perhaps than read. Whatever Time has taken from his poetry, anyone who takes up his works must still marvel at the incredible power and ease, in Shelley's just phrase, of his best and most characteristic writing. But what precisely did Shelley mean when he wrote of the desired poem as "something wholly new and relative to the age, and yet surpassingly beautiful"? What was in his mind? And what was it in "Don Juan" which could make him think that Byron had there, if only in a "certain degree," fulfilled the desired conditions?

Some men only admire what is akin to their own natures; others give their fullest admiration to qualities which they themselves do not possess. Shelley, with his native generosity and modesty, was always impelled to appreciate, and often to over-estimate, achievements of which he knew himself to be incapable. He admired, no doubt, in "Don Juan" the amazing power with which Byron poured out his thoughts and feelings, swerving off from his story and then returning to it, changing his mood on every other page, restless but always alive, witty, reckless, buoyant and abounding. Here was the modern world reflected in a modern mind, failing, no doubt, of surpassing beauty, but at any rate surpassingly vigorous and brilliantly various—something which might be called "wholly new and relative to the age."

Shelley is not the only poet who has cherished this dream of creating a poem on an adequate scale which should draw its material or its inspiration from the life of his own day. His phrase "relative to the age" is a comprehensive one; it need not by any means imply the actual portrayal of contemporary life, but it does imply the embodying in some shape of the motive forces of contemporary life, the ideas, the thoughts, the emotions actually stirring in the world about us. And Shelley, in his own way, did this, though I fancy he might have desired, had he felt himself possessed of the special powers, to find a theme in the world of his own generation, without recourse to the disguise of myth or symbolism. But natural instinct impelled him on other paths; and we do not wish it otherwise.

The time into which Shelley was born was one of those periods peculiarly apt to evoke the desire he expressed. It was a time not only of tremendous events, involving all Europe—it was a time of birth and change, of suddenly expanded horizons, of mental and spiritual ferment, of immense tragedies, out of which rose immense hopes.

It is our lot to be living in a like period, though it has come upon us abruptly, with the suddenness of an explosion. Shelley grew up as a boy in a world already changed by the French Revolution, with its profound reverberations. But for us, however sinister the omens in the recent past, the incessant piling up of armaments, the threats and the rattling of sabres, there was no real preparation. We are in a world we had no dream of, four short years ago. Again we share in immense tragedies, again we nourish immense hopes. The horizon, for each of us, has infinitely expanded: for now not only Europe but this whole planet is in the crucible fire. Events are on the scale of prodigy and legend. The whole future of the world is the issue at stake. Never was the will of man so imperiously called on for its whole effort, to the last ounce of energy and endurance. Elemental forces stream about us and carry us with them, whether we will or no.

With all its desolations, with all its bestial ferocities, with all its miseries and overshadowing terrors, this is a time which has liberated the spirit of poetry in mankind. When men live deepest and most intensely, poetry is born. If horror and revolt at the nightmare cruelties we witness are passionate, faith is passionate too. If we have seen incredible destruction, we also will a new creation. If we have laid waste, we have also sown. The very immensity of loss and sacrifice drives us inward to the spirit. And it is the spirit of poetry which transmutes our sorrows, which gives us vision of realities transcending the actual scene, which lets us breathe the air of the world of our desire, and expresses that desire for those in whom it struggles for articulation.

But with a time so overwhelming, with events following on each other so breathlessly, and a world changing before our eyes, how, it may well be asked, can poetry deal with the least adequacy? I do not think it can, and therefore I do not mean to dwell long on the relation of poetry to the war. It is too near, poignant, and absorbing.

But even before the war there was a stir and movement in the poetry of the day, a vigorous new productiveness, which seemed to mark the coming of a new era in our long poetic history. And in this movement, with its ferment and restlessness, there is an effort to relate poetry more nearly to the life of our own time. Whether poets make this effort or not, there is always a large public to demand it of them. You may remember Ruskin's complaint to Tennyson that he had not chosen in his "Idylls of the King" a subject nearer to the interests of his own time. That complaint, that demand, is heard now with increasing frequency. It may be that this is in great part due to the ever-increasing power of the newspaper Press. Journalists would like all the world to become like themselves, for whom yesterday is already the exhausted past. They clamour for actuality, for the thing which thrills us on Saturday and is stale or forgotten by Monday. Such cries, however vociferous, may leave us calm. And yet there is something both reasonable and instinctive behind this demand, though it is usually expressed so unintelligently as to become false and foolish.

The mistake made by journalists and those who are misled by journalists is simply the mistake of assuming that contemporary life consists only in its surface and appearance. Now it is obvious that the world we live in has enormously changed in outward aspect and in material conditions during the last hundred years. The discovery and application of steam-power and electricity have brought remote things near, have quickened the pace of existence, have changed the face of the country, and created a world of machinery. In the last ten years man has at last conquered the air and won a new sense of mastery over the elements. And in these last four years who shall compute the effect of the profound and vital changes in the world and in ourselves—changes material, moral, spiritual—through which we have passed and are passing every day and every hour? What account is poetry to make of a life so different from anything the world has yet seen? Might it not be plausible to claim that a new kind of poetry should emerge, if it is to be an adequate expression of this age and of its spirit? How much truth is there in this claim? And in what sense can poetry be new?

These are the questions which I should like to ask you to consider with me; and let us try to approach them, as far as possible, without prejudice. That is difficult, for we are all of us, I fancy, biassed by our natures; and while some cherish the great traditions of English poetry so ardently that they are loth to accept innovations, others are for boldly throwing over the past, shaking free from the tyranny of the "dead hand" and striking out into the future. Still, let us make the attempt; also, let us consider the question from the practical point of view—I mean from the point of view of the actual writer. Those of us who have practised any art must have experienced at one time or another the good offices of friends and acquaintances who have come to them brimming over with an ideal subject which they have discovered—"just the thing for you," as they say—which they want you to turn into a poem or a picture. But nine times out of ten the proffered subject, which seems to our friends so suitable, is one which, if they had any practical experience, they would know to be useless.

Not so very long ago there was a time when certain leaders of thought looked forward, I imagine, in their hearts to the speedy coming of an age when poetry should altogether be improved off the earth. Poetry in this view was a pleasing but useless kind of decoration, valued up to a certain stage in human development, but without value for the fully-developed race, quit of all superstitions, and living in the full day of scientific knowledge. Herbert Spencer, in his book on education, notes how the savage prefers decoration to utility, and he pours out his scorn, if I remember rightly, on the absurd importance still given in modern education to the Greek mythology compared with the deplorable lack of instruction in the interior economy of the body. To minds of his temper and training it may well seem ridiculous and lamentable that a professedly educated person should know about the Labours of Hercules—so little calculated to equip him for the battle of life—and be ignorant about the labours—so much more interesting and beneficent—of the gastric juices.

I am far from meaning to hint that, however shallow the appeal to utility may be, the prevalent ignorance of natural science is not deplorable. It should surely be a part of humane education that we should be taught something at least about the universe we live in, what things are made of, the laws by which they subsist, and the perpetual transformations of matter. These things are not antagonistic to poetry; on the contrary, they feed the springs of poetic thought and contain the elements of what may well be an increasing inspiration in the poetry of the future, not perhaps as direct themes, but indirectly by widening and deepening our conceptions of the world. But no less would I oppose the notion that the myths of mankind, and especially the pregnant myths of Greece, are mere lumber and decaying matter—as Carlyle picturesquely described them, "innumerable dead dogs upon a dunghill." Many of us are prejudiced against such subjects as themes for poetry because they have a sort of inherited and unfair prestige, and also because we incline to think them exhausted; we are tired of nymphs and heroes. And then a poet like Keats arises, who sees those old stories all fresh, and draws from them new meaning and new beauty. Or, again, in our own day, another poet, Mr. Sturge Moore, in the "Rout of the Amazons" and "The Centaur's Booty," takes up subjects which seem as remote as they well could be, and yet makes them relevant to life and in a way contemporary with ourselves.

If anyone supposed that poetry, and the desire for poetry, were obsolete in the modern world, the war should have undeceived him. Deep emotions craved for expression, and the wish to have those emotions made articulate in poetry, as the only adequate expression of them, was felt and shown among whole classes of people quite unused to literary interests. And this was surely a remarkable thing. Recall the two other great times of crisis in our national history: the struggle against Philip of Spain, the struggle against Napoleon. I do not know what contemporary demand was made on our poets in those two eventful times, both of them periods of great splendour for English poetry. But at any rate there was very little direct reflection in poetry of the national emotion. Drayton, in the time of the Spanish Armada, sang of Agincourt and Henry V. It was left for Tennyson to sing of Sir Richard Grenville and the last fight of the "Revenge." Imagine how our journalist-critics, if they had flourished in those days as they flourish now, would have scolded Shakespeare for wasting his gifts on the outlandish stories he took for his tragedies, instead of making poetry out of the heroic England of his own time! In the Napoleonic period we have a few fine ballads by Campbell. We have the famous stanzas on Waterloo in "Childe Harold," Wolfe's "Burial of Sir John Moore," and a poem by Scott which no one now reads. It was left for Thomas Hardy in our own time to make a great poetic picture of the vast European struggle. It is true that we have the noble sonnets of Wordsworth. But I wonder how many readers those sonnets then had, or how much they meant to the nation at the time. Far less, I think, than they mean to us at this moment. It is our generation, in its hour of trial, that those sonnets have strengthened and sustained. We get weary of the most vivid descriptions of the battle-field: but we do not weary of the strain of sonnets like that to Toussaint l'Ouverture—

> "Toussaint, the most unhappy man of men!
> Whether the whistling rustic tend his plough
> Within thy hearing, or thy head be now
> Pillowed in some deep dungeon's earless den;
> O miserable cheftain, where and when
> Wilt thou find patience? Yet die not! Do thou
> Wear rather in thy bonds a cheerful brow.
> Though fallen thyself, never to rise again,
> Live, and take comfort! Thou has left behind
> Powers that will work for thee, air, earth and skies:
> There's not a breathing of the common wind
> That will forget thee. Thou hast great allies:
> Thy friends are exultations, agonies,
> And love, and man's unconquerable mind."

What does this testimony of experience indicate? It shows for one thing that poets do not instinctively turn to the events of their own day as subjects for their song; rather, that their instinct is to choose events at some distance from themselves. Yet we need not assume that they are indifferent to contemporary things; and there is no doubt that the overthrow of the Spanish attempt upon England, the discovery of America, and the French Revolution were events which quickened and heightened imaginative emotion and the sense of life, and so were a stirring influence on poetic minds. But it is in the interior life that poetry springs; it was the spiritual issues, not the pageant of events, which moved Wordsworth so profoundly in the time of our struggle with Napoleon. And the spiritual issues are permanent, not conditioned by transient circumstances; they are inherent in man's nature. That is why we turn to Wordsworth to-day, and will turn to him again.

We may discard, then, the journalistic cry that poets must take their themes from the outward life about them. But if the world has changed so greatly in our time, have these new elements affected our interior life, the life of the spirit and the imagination out of which poetry is born? That is a question which it is extremely difficult, without the due perspective, to assess. There is so much that is imponderable, so much that eludes our probing. But we can try and see what tendencies are asserting themselves in the poetry of to-day; what new inspirations, if any, are behind it.

Change is the secret of life; and with a new generation, showing a remarkable abundance and variety of gift, a reaction from the aims and methods of the last generation was inevitable. It may, indeed, be maintained that this reaction, or progression, is wholly literary in essence, and had nothing to do with changes in the spirit of the age. I do not think that is true; but certainly we have to be on our guard against those numerous people who want to exploit poetry in the interests of science, or religion, or democracy, or some other cause which, however noble or desirable, is not poetry. Such people are apt to exaggerate the claims of the age, and also to attach an irrelevant value to poems which happen to interest them for non-poetic reasons. Yet it is true that poetry loses in sap and savour when it is pursued as a cloistered activity and an esoteric dream. And I think that we shall find that the tendencies of the day, subtly, and perhaps often unconsciously, embody a desire to make poetry an expression of contemporary life.

The Victorian epoch closed a period in poetry which now returns to conditions more like the conditions prevailing a century and more ago, in the time of the Romantic Movement. But just as the Victorian poetry was far more complex and varied than that of the 18th Century, so the reactions of our day are more complex than the reaction of Wordsworth and Coleridge; they cannot be summed up as a single movement. We have to deal with a numerous company of poets, widely differing in gift and temperament. Yet certain tendencies are manifest.

Changes have appeared both in the content and form of poetry: in choice of subject, in mood of approach, in diction and vocabulary, in rhythm and metre.

To begin with the question of subject-matter. The Victorians were varied enough in the themes they handled; but poetry is still expanding its frontiers. It would be rash to assume that there is any subject-matter which an adventurous poet of to-day would refuse to handle. There is a determination abroad that poetry shall come to grips with realities. And in this quest there is to be no flinching from the ugly, the horrible, the squalid. Literary historians of the future may see in this a consequence of the realism of the later 19th Century, itself a reflection from the rise to dominance in current thought of natural science—the realism that first overflowed the novel and now spreads to poetry. Whether this is true or not, we find in many, though not all, of the poets of to-day a wish to deal with things as they are, and when the thing is, to say the word. There is a decided wish to avoid the suave, the soft, the pretty. Ugly and prosaic subjects will seem to be deliberately chosen, as in some of the earlier work of Rupert Brooke. This may sometimes be a mere reaction from convention, or a fashion; but as a positive tendency it is a symptom of life and vigour. Let us not condemn a poem for having a subject we think ugly. The sole question is, Does it remain ugly, does it remain mere fact, or has the poet been able to transmute it so that it is related to the rest of life, seen in the world, not of fact, but of ideas? The victory is difficult, but difficulty attracts an artist.

Mr. Hardy, though an honoured veteran, is a poet who is modern of the moderns. And if we are curious to see how unpromising subjects, as far removed as possible from traditional sentiment, can be transmuted into poetry, not always felicitous perhaps, but always impressive because born of intense sincerity and coloured by a very personal temperament, it is to Mr. Hardy's poems we shall turn.

The danger in this extension of the themes of poetry lies in this: Poetry is the natural expression of an intense emotion; and if themes, not inevitably evoking emotion, are attacked solely from a desire to handle reality, then the inner life of the poem flags, it loses spontaneous glow, and we feel that poetry is usurping the place of prose. But here we must distinguish. What evokes no emotion from the ordinary man may evoke deep feeling from the poet. That is his opportunity to win for beauty what had been thought outcast, common and unclean. And I do not think it can be justly said that the poetry of our day is realistic in any narrow or stupid sense. Mr. Masefield, Mr. Wilfrid Gibson, Rupert Brooke, Mr. Gordon Bottomley, and others now writing have handled themes of common, hard and ugly experience, or introduced realities called squalid into poetry. They have not always been successful; but one cannot accuse them of lacking emotional impulse or imaginative mood.

There are other poets who do not choose such themes; but even in those who keep more to the traditionally poetic world you will find a change in the mood of approach, in diction, and in versification. What you will notice is a general distaste for what is called "literary"; a distaste for the acceptedly beautiful word, a preference for the expressive over the melodious. There is a desire to take in more from contemporary speech, even what an older generation would have despised as slang. It is a desire for new blood. You will find this in such a poet as Mr. Lascelles Abercrombie, a poet exuberant alike in intellect and in imagination, whose vocabulary is singularly rich and vigorous. Along with an avoidance of the stock metaphor and image there is also an avoidance of the mellifluous rhythms in blank verse of which Tennyson was so finished a master. Just as the poets of the Romantic Movement turned away from the smoothness of the Popian couplet, the balance and neatness of

"Know thou thyself; presume not God to scan,
The proper study of mankind is man,"

so those now writing cannot return to the artifice of Tennysonian rhythms like

"The moan of doves in immemorial elms
And murmur of innumerable bees."

Mr. Abercrombie, indeed, goes to excess in his passion for variety and emphasis. So many of his lines are exceptional in rhythm that it is hard to say what the rule or norm is to which they are exceptions. This is a weakness, but it is a fault of excess; and the brilliance, the vigour, and the frequent beauty of the verse are undeniable.

Mr. Yeats has little in common with most of the younger generation; but in Mr. Yeats's verse also you will find a sedulous sifting of diction and vocabulary, with the aim of getting rid of the dead matter of poetic tradition and discovering fresh imagery. His poetry aims at a kind of transfigured homeliness and intimacy of speech. Conventions such as the placing of the epithet after the noun, and the inversion of the natural order of words, are scrupulously rejected. "You" replaces "thou," and "it's" replaces "'tis." Mr. Yeats has arrived at a simplicity far removed from the baldness which in Wordsworth's most defiant early pieces was still not free from literary conventions, a simplicity sometimes over-conscious, but at its best, as in the poem of "Baile and Ailinn," for instance, of an exquisite economy, relieved by vivid images and subtly modulated rhythm—a style that in its own kind seems to me well-nigh perfect. The example this poet has given of delicate precision, his repudiation of all the lazy artifices that come so easily to hand, and that have been too frequent in our poetry, has had considerable influence. If you prefer, we may suppose it symptomatic of the tendencies of the time.

And now to say something of changes in form. Here, again, I would plead for a discarding of prejudice in face of what is unfamiliar and may seem undesirable. When we speak of form in poetry, what do we mean? Most people, I think, use the word as meaning a strict adherence to a metrical scheme, and a regularity in the scheme itself. They have in their minds such things as the scheme of the sonnet, with its octave and sestet and its strict arrangement of rhymes. And it is true that such forms as the sonnet yield a certain pleasure in themselves apart from what they contain. They are like a fine contour or outline. But though such a given outline may help a poor writer to produce something that gives pleasure, it is not of itself living form; it is imposed from without, not shaped from within. In a good sonnet there is a vital correspondence between the matter of the poem and the shape it assumes; and this is true of all good poems of whatever form. There is what we might call an interior form in every poem which does not depend on the metrical outline so much as on the organic relation of the parts to the whole. In the perfect poem there is a complete and glowing fusion; not a syllable can be removed or changed without injuring its life. Thought, emotion, imagery, rhythm, metre are fused into an indissoluble unity. The body cannot be separated from the soul which it expresses. But the poems which attain this absolute fusion are comparatively rare. And in work which is wrought at a lesser intensity we are conscious of a certain imperfection in the relation of matter to form; and we say the matter is superior to the form, or the form to the matter. But what I want to bring out is this: that regularity of metrical structure may disguise a poor internal form, while an apparent looseness and raggedness may blind us to the solid and organic shape beneath. To take an example: Swinburne's "Ode to Victor Hugo" is composed in sonorous stanzas of a fine pattern, faultless in metrical structure. Walt Whitman's "Song of the Broad Axe" is also an ode; but it is not metrical, its rhythms are irregular; it shows no apparent system; and at first blush it would seem reasonable to say that in point of form Swinburne's ode has every superiority over it. But read these two poems over, and aloud; forget the printed page; think only of what impression has been left on your mind. I shall be surprised if you do not find that Whitman's poem, which has after all a rhythmical recurrence of imaginative motives, does not seem to have much the more organic vigour of structure and richness of interior form. It is true that probably this impression will be stronger when you no longer retain the actual words of either poem in your mind; and that indicates the defect of Whitman's poem, which is encumbered by roughnesses and superfluities of language. On the other hand, Keats's "Nightingale," which is a case of that absolute fusion I spoke of a moment ago, loses something with every word of it that the memory fails to hold.

The impulse that makes a poem ought to find its own predestined form, rather than be like molten metal run into any mould that lies handy. And if the language is to obey the impulse behind it, changing with each turn of mood, why should not the form it takes be what is termed loose and irregular? In vital correspondence between the inner and the outer, may not such form be really stricter and cleaner? A set metrical shape tempts to fill up with superfluous matter spaces that should be left empty. At any rate let us try to get rid of prejudice in this matter. When we meet with a modern experiment that discards rhyme and metre, let us not condemn it forthwith on that account, but read it aloud and ask ourselves whether it honestly pleases or not. Few, perhaps, of these poems will satisfy us; but I do not think it is merely because of the lack of rhyme and metre. Recall Hamlet's outburst: "What a piece of work is a man! How noble in reason, how infinite in faculty! . . . In action how like an angel! In apprehension how like a god! The beauty of the world, the paragon of animals! And yet, to me, what is this quintessence of dust?"

There is no metre there, but there are rhythms of magical beauty; and if poets could always write on that level we might well be tempted to think metre, except in pure song, an outworn and inferior mode. Unfortunately, to write poetry without metre exacts a higher discipline, a stronger inspiration, and a severer sense of form than to write in metre. Only those who have mastered all the secrets of metre are really competent to discard it. But naturally this free verse, whether modelled on Whitman or on French examples, attracts by its apparent ease; and we find it most often used for little pieces of picturesque impression, rapid sketches such as an artist jots in his note-book, where the emotional impulse is not strong or profound, and the result is something accidental, slight and ragged. There is no resistance in the medium, and the absolute freedom of the writer tempts even more to laziness than the prop and support of a set metre. The test of this kind of verse is whether or no we feel that it could not have found another form. And I think this is true of certain poems by our soldier-poets, such as Mr. Robert Nichols and Mr. Frederic Manning—intense impressions of the atmosphere of battle. But I do not believe that, poets being what they are, there is a great future for this type of verse. On the other hand, I believe that there is a rich country to be explored in the field of rhythm. Certain traditional rhythms associated with noble verse are, for the time at least, exhausted. In this exploration, and in the winning over to verse of the infinitely various rhythms of speech, Mr. Bridges has been a leader of stimulating example. His "London Snow," his "Nightingales," and many of his more recent poems, show what quite new rhythmical effects English verse is still capable of. There you have the freedom of *vers libre*, but within that freedom an inner principle controlling and vitalizing the form. I am sorry I have no space to give illustrations, but this subject alone might well take all my time.

Just now poetry finds delight in a loosening and expansion of traditional forms, in a variety of experiments. Later, no doubt, it will return with a new joy to tighter forms and cleanly ringing metre, for it is by such change and reaction that an art lives.

I have tried to sketch some of the broad tendencies of the time in verse. If we ask whether the poetry of to-day expresses the vast changes that have come upon our world of late, it does not seem that we can point to any direct relation of cause and effect, though we do see that poetry is intensely alive and full of new adventure. But let us beware of thinking that old themes or old measures—I do not say of old rhythms within those measures—are easily exhausted. To a poet it is a matter of wonder not that so much has been made of old themes as that, comparatively, so little has been made. He is amazed at the apathy and incuriousness of the average man, who needs some grossly tangible portent like the coming of the steam-engine or the aeroplane to rouse his sense of wonder, and then it is but for a day or a week. With all our gains in knowledge and widening of interests we turn back continually to certain central and primary interests which have prompted spontaneous poetry since articulate speech first brimmed over into song. The relation of mother to child, the passion of love, death and birth, the passing of the seasons—such themes are perennially new because of that greatest of wonders, the uniqueness of human personality. Each brings to that old experience his own newness, just as if no one had felt those feelings before. Among the poems of our own day what is more delightful than the best of the songs of Mr. W. H. Davies, who sings of his joy in the simplest sights and sounds—birds and flowers and the rain on the leaves—with such freshness that the commonest experience becomes a rare emotion? Or read Mr. Ralph Hodgson's "Song of Honour," and see on what vibrating strings the mere sense of life intensely lived can evoke a new and memorable music.

So far I have spoken of poetry as the expression of thought and feeling; but the real difficulties for the modern poet begin when, not content with expressing his own thoughts and emotions, he aspires to create on a large scale and sets out to rival the masterpieces of the past. Since the 18th Century there has been no lack of great poets in England and elsewhere; but how few comparatively are the great poems! Everyone must be conscious of a disparity between prodigality of gift and inadequacy of performance. I am of course excluding lyric poetry, in which the modern period has been so abundant and so splendid. I am speaking of poems on the grand scale. England had a galaxy of great poets in the 19th Century, but none of them produced a work of the type I mean which we can rank, in quality of success, with their smaller works. It may be maintained that lyrical poetry, in the large sense of the word "lyrical," is the kind most congenial to the modern spirit, and that this is a wasted regret. Some may even subscribe to the heresy of Edgar Poe, who contended that poetry could only be at its finest in short spurts of inspiration, and that a long poem was only a series of small poems superfluously joined together by dull passages. But it is a sound instinct in mankind, and not a mere superstition, which accords the highest rank in works of human art to masterpieces on a large scale. We demand not only fitful inspiration, but that full deployment of creative faculty which can handle the most complex elements, throw them into living shape, and, transcending private experience, project a fresh world before our eyes. Only by mastery of the complex, by intuitive grasp and control of the reality outside himself, is the master fully shown. What has gone to the making of a work of art comes out of it. And the great masterpieces radiate an energy of the spirit which is a perpetual exhilaration and expansion for our minds. Who can read "Paradise Lost" and not feel dilated as with a new sense of the powers of human nature?

Well, let us imagine a poet of our day contemplating the attempt to embody all that life means for him in a great objective work. He must find a medium; he must choose a subject.

Mr. Bradley, in one of his Oxford Lectures, discussing this very question of the long poem in modern times, suggests several reasons for the frequency of its failure. One is this. The modern mind is not content with a narrative of events, however splendid and moving; it craves for some profounder interpretation of a world which grows ever more complex and limitless to our understanding. The trend is inward. We are interested in the movement of the souls and minds of men, in spiritual victory and catastrophe, rather than in action for its own sake. It is Hamlet himself, the subtle workings of his mind and its reaction on his will, who enthrals one generation after another; not the external story, with its murders and violences. Yet it would seem hardly necessary to point out that it is through his actions that Hamlet reveals himself: and what we really demand is that action should be significant, should be the inevitable issue of character in movement. If action is suppressed and neglected in a long poem, the salient outlines become flat and tame. There is a loss of vital energy, of concrete definition: the sense of mass, the sense of movement, are alike missing. Poems like the "Excursion" or "Paracelsus" show in their different ways how much reflection and analysis may overweight the poet and fatigue the reader.

I agree with Mr. Bradley that this preoccupation with the inner life is one main cause of the rarity of success in modern poems on the great scale. I agree also with most of what he says about another cause. That is the modern poet's freedom, his unchartered freedom. We live in an age when the history of the world and the surface of this planet have been explored and made known in a way that hitherto has been impossible. To a poet seeking a subject all times and countries are open. He has but to choose. Yet this necessity of choice, this unlimited field, how baffling and distracting they can be! For he feels instinctively that he must choose some subject at once related to his own nature, and to the lives of those around him. Far easier was it for the poet who lived in a more circumscribed society, in a smaller world, where his relation to his public was definite and assured. The limitation to an expected cycle of subjects concentrated his energy into certain channels, and gave it force and volume. The Greek tragedians worked in those conditions. But in times like ours the poet has to establish a relation with his audience, an audience potentially vast but actually vague. He seeks a theme which, in Shelley's words, shall be relative to the age, and in practice he finds that a hard quest.

If he consults the public, the public will probably say to him: Surely the present is more interesting than the past. Why not boldly take your theme from the life we are living now? I agree that no time is more interesting than the present; and I think most poets would like, if they could, to create something splendid and significant out of it. Perhaps some day it will be done. But for the kind of poem of which I am speaking, a concrete poem, whether narrative or ostensibly dramatic in form, there are disabling conditions in a purely contemporary theme. Two great modern poets, Whitman and Verhaeren, have sung the actual scene before them; and they have given us eyes to see realities, ignored and despised as ugly, with a new illumination. Verhaeren has made wonderful poetry out of that great and disturbing phenomenon of our times, the flight from the country to the towns; he sees it as a kind of elemental struggle between two forces. But Verhaeren does this by means of separate poems, lyrical in form, which make up a sort of whole, it is true, but are not the same as a single coherent and concrete creation. And Whitman, too, is lyrical from beginning to end. He seems to be always announcing the new great poem of democracy, which never actually comes. It is open to anyone to say that forms like the epic are dead, and that the type of poetry which we find in Whitman or Verhaeren, or some other, has taken its place. But the creative instinct in poets will, I think, always urge them at least to attempt something more, to deal with character in action on the grand scale.

Now, for one thing, modern life, and especially modern democratic life, does not tend to throw up great characters in relief, rather it submerges them in the stream. And poetry like Whitman's is almost entirely concerned with the variegated mass of average men and women; it is indiscriminate and anonymous. Before the war we might, perhaps, have been apt to scoff at Whitman's glorification of the average man. We saw him as we met him in the ordinary ways of life, and found him uninteresting. But it is the average man who is the hero of this war; there is no doubt of that. He it is who has given us a new reverence for common humanity; what he has borne and done surpasses imagination to conceive. Yet it is just the numbers, the namelessness, of the common man that so deeply impress us. And if the poet takes him for a hero—I am speaking now from the practical point of view—what is he to do?

If he takes an individual and makes of him a living character, the more his story is occupied with him as a person, the less will the significant sense of that swarming anonymous mass of human beings, of which he is but a unit, pervade the poem. If he remain a type, he will be colourless and lack life. And in any case, so much of him must be passive; he is caught into great actions and events, of whose issue and meaning he is ignorant. Here are difficulties possibly not insurmountable, but certainly intimidating, since the poet will seek above all things for vigour of design and salient relief in his theme, and all this tends to a blurred outline.

The material does not run naturally into the mould. Rather it is the modern novel which is the natural, the inevitable form for such a theme.

But suppose that we avoid the common man, however much a hero. Suppose we take individual characters whose actions are really pregnant and far-reaching, in whom the asserted will makes for happiness or calamity. If we take known persons, we cannot deal with them freely; they are too familiar on the surface, so that a hundred irrelevant criticisms and curiosities are roused in the reader, and yet in their inner selves, that which has most interest for the poet, they are too little known. The portrayal will either be superficial or fanciful. And if we take imaginary persons of our own creating, we are again in the field of the modern novel. All the detail of modern life, the detail we want to have forgotten, for the sake of the spiritually significant, the inner reality, suggests itself inevitably to the reader. Prose can handle this easily; but it is a clog on poetic movement.

I think these reflections are enough to suggest why poets have instinctively avoided in poems of a large scale actual contemporary life. Let us go back for a moment to the example with which we started. You remember how "Don Juan" opens—

"I want a hero; an uncommon want,
When every year and month sends forth a new one."

How truly might we echo that in these days! Then follows a list of eminent men of action of Byron's time, including Wellington and Nelson, all dismissed as

"Exceedingly remarkable at times,
But not at all adapted to my rhymes."

They are rejected for "our ancient friend, Don Juan." It is true that Don Juan is re-created and made a contemporary of the poet's, but through what remote scenes of adventure and romance he is led! What is really contemporary and relative to the age is Byron's mocking comment, not only on his theme, but on every matter that comes into his head. The poem grows less and less concrete as it proceeds; it is unfinished, but we hardly notice that, it has so little of objective shape and form. And, in truth, one might ask whether it is really as relative to its age as Shelley's "Prometheus," which under a mythic disguise embodies the hopes of that rarefied yet passionate idealism which was so characteristic of the time. With all its splendour and its incomparable lyrics, Shelley's "Prometheus" does not rank as a solid richly structural achievement with the great poems it attempts to rival. Yet I do not think it fails because its nominal theme is an old Greek legend. I think such a theme is likelier of success than such a theme as Wordsworth took in his "Excursion," which, again, though sown with beauties and often exquisite in its felicity of language, makes as a whole a flat impression: the characters are so stationary, and their reflective talk so much a monotone. Of all modern English poets Keats had the gifts most adequate for the task. He had grandeur and simplicity of conception; his imagination was sensuous and concrete. But he broke off his "Hyperion" because the subject dissatisfied him: he tried, in recasting it, to give the subject a more real relation both to himself and to his age—to make it significant, and in a deep, though not superficial sense, modern. It is certainly a misfortune for us in England that we have so little in the way of national myth and legend to draw upon; for in the far-descended stories that come down from a nation's childhood there is usually embodied something innate and permanent which belongs to the character of the race; and, being fluid rather than fixed, they are material which every generation of poets can take up and fashion anew and adapt to its own uses. They are removed from actual history, so that the impertinent antiquarian cannot intrude his idle stickling for the facts of time and place. We have the Arthurian legends, it is true, but these have come to us in a version already modernized and mediævalized, so that the truth of their pristine features is overlaid with the colouring of an artificial age, with courtly codes and romantic decorations, from which it is hard now to extricate the vital elements. But if anyone doubt the power of mythic stories to stir the blood of the race in whose imagination they were born, let him consider what Wagner's treatment of the cycle of Northern legends has meant to the Germany of our day, or what the Irish legends, resuscitated in recent years and handled in poetry by Mr. Yeats and his followers, have meant to the youth of Ireland.

It is to such material as this that a creative poet turns by instinct. He wants to be at a certain distance in time from his theme, because he wants to grasp it clearly as a whole, because he does not want to be distracted by externals and irrelevant, but none the less insistent, associations; because he wants to concentrate on human spirits in action; because he wants figures of a strong outline and a pregnant type; because he wants to get away from the world of fact to the world of imaginative reality. He does not go to the past for the sake of what is past, but for the sake of what is permanently living. He asserts the continuity of human history, just as, in the light of science, confirming his own intuitions, he will assert more and more, I believe, the continuity of all life, the continuity of the universe.

I may seem to have expressed a despondent view on the possibilities in England for a poet who seeks to create on a large scale a work that shall be no refuge of romance, no product of a cloistered studio, however beautiful, but something essentially related to his own age. But poets have their ways of discovering themes. I believe in the future of English poetry. Only the other day a long poem was published which found a theme at once new and relative to the age—I mean the "Song of the Plow," by Mr. Maurice Hewlett—a poem sustained with so intense an energy that it conquered matter the most intractable, a poem of good augury and fine incentive, telling of the past and breathing of the present.

Mr. Bradley, in the lecture I referred to, tentatively suggested one hampering condition for the modern poet who would celebrate great actions—that his life was private and sequestered and unacquainted with the world of action. Of our younger generation that can no longer be said. Rather it might be said that the world of action and stress is too much with us, that poets lack the stillness in which, according to Goethe, genius grows best. But let us have faith. We cannot predict what forms poetry will take, nor what themes it will choose. Poetry, genuine poetry, will always be new, because it brings—what is so much more precious than novelty of subject—the eternal freshness of unique personality, the poet himself.

[L. B.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1918.

Sir James Crichton-Browne, M.D. F.R.S., Treasurer and Vice-President, in the Chair.

The Chairman announced the decease of His Grace the Duke of Northumberland, K.G., President, on May 14.

The following Resolution of condolence was submitted to the Members, and unanimously passed :—

The Members of the Royal Institution of Great Britain, in General Monthly Meeting assembled, desire to express to His Grace the Duke of Northumberland their sympathy with him and his family in the loss which they, as well as the Royal Institution and the Country at large, have sustained by the death of his father, the late Duke.

They desire to record their grateful sense of the invaluable services rendered to the Royal Institution during the late Duke's Membership of 34 years; a period in which he held office as Manager on four occasions, and as the President for 19 years. His Grace's active interest in the progress of Science, which it is the object of the Royal Institution to promote, together with His Grace's nobility of character, have lent distinction to its proceedings during an eventful period of its history.

The Duke of Northumberland,
Arnold Jacob Cohen Stuart,
William Rintoul,
were elected Members of the Royal Institution.

Report from the Managers, June 3, 1918 :—

The Managers reported that the Fullerian Professorship of Chemistry would become vacant on June 7, and that in pursuance of the Deed of Endowment the election of a Professor by them would take place on July 1 next.

The Chairman declared, in the terms of the Bye-Laws, Chap. 4, Article 2, there was a Vacancy in the Office of President through the decease of His Grace the Duke of Northumberland, K.G., and a vacancy in the Office of Visitor through the decease of Sir Alexander Pedler, and at the next General Meeting, on July 1, the Vacancies will be filled in accordance with the said Bye-Law.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Electrical Times for May, 1918. 4to.
Electricity for May, 1918. 8vo.
General Electric Review for May, 1918.
Horological Journal for May, 1918. 8vo.
Illuminating Engineer for March, 1918. 8vo.
Junior Mechanics for May, 1918.
Law Journal for May, 1918. 4to.
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Nature for May, 1918. 4to.
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WEEKLY EVENING MEETING,

Friday, June 7, 1918.

The Hon. Richard Clere Parsons, M.Inst.C.E.,
Vice-President, in the Chair.

Sir Boverton Redwood, Bart., D.Sc. F.R.S.E. M.R.I.

The Romance of Petroleum.

Petroleum is defined in the Petroleum Act of 1871 as including "any rock oil, Rangoon oil, Burmah oil, oil made from petroleum, coal, schist, shale, peat or other bituminous substances, and any products of petroleum, or any of the above-mentioned oils."

The scientific definition is even wider, embracing natural gas, solid bitumen and ozokerite.

It is, therefore, an appropriate introduction, for the suggestion of which I am indebted to the Fullerian Professor of Chemistry, to recall that it was in the Laboratory of this institution, in 1825, that Faraday examined the liquid which separates when the gas made by the destructive distillation of fixed oils is subjected to compression, and isolated from it the hydrocarbon benzene, as well as several other compounds of carbon and hydrogen.

In 1815, John Taylor was granted a patent for a process described as producing "inflammable air or olefiant gas applicable to the purposes of giving light" from vegetable or animal oil, fat, bitumen, or resin. This oil-gas, compressed by a method patented by Gordon and Heard in 1819, was supplied by a company having the title of the London Portable Gas Company. It was contained in vessels having a capacity of two cubic feet, which were delivered to the premises of consumers, and returned when empty to be refilled. The liquid which separated when the gas was compressed into these cylinders was that which Faraday examined.

It is not reasonable to assume that whilst he was ascertaining the chemical constitution and properties of what was actually synthetic petroleum, Faraday can have realised the importance of the part destined to be played by these hydrocarbons in peace and in war.

Nevertheless, his extended reference to what he describes as the remarkable action of sulphuric acid upon the compounds of carbon and hydrogen which he had isolated, and his subsequent paper on the mutual action of sulphuric acid and naphthalene, appear to indicate that he may have had an intuitive perception of the valuable industrial developments in the manufacture of dyes, which after many years followed his classic researches.

In the same year, Faraday also published the results of his examination of caoutchouc, and showed that this substance is mainly a compound of carbon and hydrogen.

Eleven years later, Edmund Davy, a cousin of Sir Humphry Davy, discovered the gaseous hydrocarbon which we now know as acetylene. The account of his discovery which he gave at the meeting of the British Association in 1836 was as follows: "Early in the present year the author, in attempting to procure potassium, by strongly heating a mixture of calcined tartar and charcoal in a large iron bottle, obtained a black substance which readily decomposed water and yielded a gas which, on examination, proved to be a new compound of carbon and hydrogen."

It is interesting to note the relation between these respective researches of Faraday and Edmund Davy and the rival theories of the organic and inorganic origin of petroleum, to which further reference will be made.

As an illustration of unexpected issues of scientific investigation, I may remark, though I must do so in carefully guarded terms, that a hydrocarbon, largely employed in early low-temperature research work in this Institution, is now being made use of by the enemy in one of the most abhorrent forms of modern offensive warfare.

There are many obvious allusions to the occurrence and uses of petroleum in the Old Testament scriptures. Thus, in the account of the building of the Tower of Babel we are told that "slime had they for mortar," the word "slime" in our version being given as "bitumen" in the Vulgate. Again, in Genesis xiv. 10, the Vale of Siddim is described as "full of slime pits," and on this account it has been suggested that the destruction of Sodom and Gomorrah may have occurred through the sudden outburst of petroleum in this region. This has led Mr. W. H. Dalton to remark that the destruction of these cities, and our recent conquests in Palestine, were effected by the same agency, with the essential difference that in the latter case the flow of the oil was under control.

The Vale of Siddim, with its slime pits, is no more; even its precise position is a matter of doubt, but the pitch spring of the Ionian island of Zante, described by Herodotus in 450 B.C., may still be seen.

Here is a photograph (Fig. 1) of this spring of petroleum taken in 1890, whilst my guide was in the act of inserting an olive branch into the spring, and withdrawing it dripping with the oil, the flow being, apparently, as abundant as it was 2,300 years previously. I may add that drilling for petroleum in the locality has not resulted in obtaining any yield of commercial importance.

Long before the Christian era, the drilling of wells for natural gas, with a view to its use as a source of heat in evaporating brine, was a recognised industry in China, and it is worthy of note that the instruments employed bear a close resemblance to modern drilling appliances.

Petroleum occurs in greater or less quantity throughout the whole range of strata of the earth's crust, from the Laurentian rocks to the most recent members of the Quaternary period, but it is found in quantities of industrial importance almost wholly in the comparatively old Devonian and Carboniferous formations, on the one hand, or in the various divisions of the comparatively young Tertiary rocks, on the other.

Its origin has been the subject of much controversy among distinguished geologists and chemists who have devoted special study to the subject. Berthelot and Mendeleeff lent the weight of their authority to the theory that petroleum was derived from metallic carbides lying far beneath the porous strata in which the oil is stored, and made the attractive suggestion that the process might be conceivably in operation at the present time. The view is now, however, universally accepted that petroleum is of organic origin, and that it has been produced from vegetable matter and the lower forms of animal life, chiefly aggregated during the geological periods referred to, when favourable conditions, which did not persist through the whole period, occurred. In certain places, for instance in Karabugas Bay, on the eastern side of the Caspian Sea, in Sweden, in Sardinia, and in the eastern part of the Mediterranean, there is some conversion of organic matter into petroleum actually to be seen in progress at the present time.

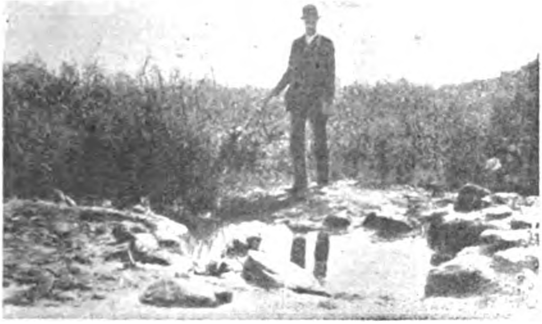

FIG. 1.—ZANTE. OIL SPRING.

It is not difficult to account for the formation of adequate deposits of the necessary material. In the comparatively deep and quiescent water along the margin of the land in past ages there would be abundant opportunity for the deposition not only of the remains of marine animals and plants, but also of vegetable matter brought down to the coast by the water-courses, and the changes which the earth has undergone would result in the burial of these substances under sedimentary mineral matter, the deposits thus formed being ultimately, as the result of further alterations in the earth's surface, often found occupying positions far removed from the sea, and sometimes beneath immense thicknesses of subsequent deposits. The instance of Karabugas Bay, already referred to, affords an excellent illustration of the manner in which the necessary accumulations of marine remains probably occurred. In this land-locked bay organic matter is being constantly conveyed from the open sea through a very shallow entrance over the bar, and as the water of the bay is rendered more strongly saline by the active evaporation which occurs in this torrid region, all life is quickly destroyed, whilst at the same time putrefactive decomposition of the remains is prevented.

Whilst, however, as I have said, there is general agreement as to the organic origin of petroleum, there is considerable difference of opinion as to whether the oil is in all cases indigenous to the strata in which it is found, and as to whether the conversion of the organic matter was practically completed when the strata were formed, so that the age of the rocks is that of the petroleum found therein. There are distinguished advocates of the view that petroleum results from the action of a slow continuous process of distillation of the material yielding it, accompanied by a transference of the product to strata lying above those in which its formation originated. According to some, this process occurred at a definite and distant time in the past, long subsequent to the formation of the petroliferous strata; but in the opinion of others it may be in progress at the present time. The question is not one of academic interest only, for it obviously would be of vast importance if it could be demonstrated that our stores of petroleum, which are being depleted with alarming rapidity, might be replenished. I fear, however, that there is no ground for such an encouraging anticipation. As Lesley, the United States geologist, remarked in 1886, "I am no geologist if it be true that the manufacture of oil in the laboratory of nature is still going on at the hundredth or the thousandth part of the rate of its exhaustion. And the science of geology may as well be abandoned as a guide if events prove that such a production of oil in Western Pennsylvania as our statistics exhibit can continue for successive generations. It cannot be. There is a limited amount. Our children will merely, and with difficulty, drain the dregs."

Probably each of the views expressed in relation to the organic origin of petroleum has some elements of truth in it, and it is reasonable tc assume that a substance so varied in chemical and physical characters has not in all cases been created under precisely the same conditions or from an exactly similar source. On the whole, however, the balance of evidence appears to point to the conclusion that the petroleum which we now find in the Palæozoic and Tertiary rocks is of substantially the same geological age as the rocks themselves. It is, I believe, uncertain whether man existed on the earth before the close of the Tertiary period, though there appears to be no valid reason why he should not have, but there is abundant evidence of the existence of the human race in the following Quaternary period. The advent of man may, therefore, have been coeval with the completion of the petroliferous formations.

The following two slides are reproduced from illustrations in Pouchet's work on "The Universe," and may be considered to give some idea of what the surface of the earth was like during the two principal periods of the creation of petroleum. The first is an imaginary view of a forest of the Carboniferous period. At this time the earth had long passed the stage of being "without form and void." The elementary and compound substances of which it is made up had ceased to exist countless ages before in their original condition of incandescent gases and vapours; the contractile movement of the earth had resulted in the formation of valleys on the outer crust of which the condensed aqueous vapour collected as seas; and the land was covered with luxuriant vegetation. In the foreground of the picture is a swamp which, presuming its proximity to the sea-coast, may be regarded as a typical birthplace of petroleum. Pouchet says of this period that the whole surface of the land was covered with strange and dense forests, where proudly reigned a host of plants the representatives of which at the present day play but a very humble part. These vast primeval forests, which the course of ages was to annihilate, sprang up on a heated and marshy soil, which surrounded the lofty trees with thick compact masses of herbaceous aquatic plants, intended to play a great part in the formation of coal. This rich covering of vegetation, which extended from pole to pole, was sad and silent, as well as strangely monotonous. Not a single flower enlivened the foliage, not one edible fruit loaded its branches. A sky, ever sombre and veiled, oppressed with heavy clouds the domes of these forests. A wan and dubious light scarcely made visible the dark and naked trunks; on all sides reigned a shadowy and indescribable hue of horror. The echoes remained absolutely mute, and the branches without a sign of life, for no air-breathing animal had as yet appeared among the savage scenes of the ancient world. Of only one reptile have remains been found, but in the seas were abundant fish and shelled molluscs.

The next slide indicates the conditions which prevailed during the Tertiary period, when the formation of our stores of petroleum was being completed. In this epoch peaceful and luxuriant nature was, as Pouchet expresses it, animated for the first time with inoffensive mammals, some of singular forms, others of colossal size. The vegetation of this period exhibits a great analogy with the present flora of the temperate regions of the northern hemisphere, and in the seas were new races of molluscs in great quantity.

Not less important than the provision of adequate supplies of organic matter to be transformed into petroleum is that of a suitable rock-formation for its reception and conservation. For the latter we need a porous stratum, such as coarse-grained sandstone or conglomerate or dolomitised limestone, with an impervious cover, such as that provided by fine-grained shale. In addition, in order that the wells drilled may furnish individually a large yield of oil, it is essential that the petroliferous strata should have been caused to assume an anticlinal structure. Under these tectonic conditions any natural gas accompanying the oil accumulates at the crest of the anticline, whilst the oil occupies the flanks, and water is found in the synclines. The gas often occurs at a pressure of many hundred pounds on the square inch, and it is obvious that in these circumstances a well drilled into the flank of the anticline may produce an oil fountain.

The geographical distribution of petroleum is no less wide than the geological, as will be seen from the next slide, whereon the principal occurrences are marked in red, but the deposits mainly occur along well-defined lines, often associated with the mountain ranges. This is chiefly due to the formation, in the elevatory process, of minor folds, which have arrested and collected the oil in richly-productive belts, between more or less barren areas, in the manner already referred to.

There are, however, but few of the localities indicated as petroliferous which contribute largely to the world's output of petroleum, estimated for last year as approximately 70,403,128 metric tons. This is shown in the following slide.

The predominant contributor, as will be observed, is the United States, which furnished no less than 64·74 per cent of the estimated total for 1917, the others in order of importance being: Russia, with 13·26 per cent; Mexico, with 11·37 per cent; the Dutch East Indies, with 2·74 per cent; Roumania, with 2·08 per cent; India (Burma and Assam), with 1·61 per cent; Persia, with 1·32 per cent; Galicia, with 0·947 per cent; Japan, with 0·615 per cent; Peru, with 0·511 per cent: Trinidad, with 0·303 per cent; Germany, with 0·189 per cent: the Argentine, with 0 170 per cent; Egypt, with 0·094 per cent; Canada, with 0·037 per cent; Italy, with 0·002 per cent; and other countries, with 0·006 per cent.

It is not surprising that the flood of oil which has been poured out by the wells of the United States in ever-increasing volume since 1859 should now be attended by signs of the approaching exhaustion of the petroliferous territory, and it has been estimated by Dr. David T. Day that, at the present rate of increase of the output of petroleum, the known oilfields of that country will, on the basis of the minimum quantity of oil obtainable, be exhausted by the year 1935.

It will be readily understood that the temporary storage and the transport to the refineries of so large a volume of crude oil as is represented by the annual output of over 70 million tons necessitates the provision of facilities on a scale of great magnitude. In the United States, and in many other oil-producing countries, the storage is effected in vertical cylindrical steel tanks, which are usually 90 feet in diameter by 30 feet in height, whilst in Russia excavated earth reservoirs, often of great size, are also employed. It will assist us to realise what is needed if I give you some calculations which have been obligingly made for me by Mr. William Sutton.

A vertical cylindrical tank 30 feet in height would need to be 10,952 feet, or a little over two miles, in diameter to hold the world's production of petroleum for last year. Such a tank would cover an area approximately three times that of Hyde Park and Kensington Gardens together.

It would require a pipe 6 feet 2 inches internal diameter, with a rate of oil-flow of 3 feet per second, or two miles an hour, delivering continuously throughout the year, to transport the world's production by that means.

The oil would fill over seven million 10-ton railway tank waggons. These would make a train of 28,000 miles in length, considerably more than sufficient to encircle the earth at the equator, and such a train, running at 25 miles an hour, would take 46 days 16 hours to pass a given point.

The world's production of crude petroleum is equivalent to 17 gallons *per capita* of the estimated population of the world.

The pipe-line system of transport of the crude oil from the various oil-fields to the seaboard, where the chief refineries are situated, over a distance of several hundred miles, has been brought to great perfection in the United States, and there are now thousands of miles of trunk lines, up to 8 inches in diameter, in operation.

The working pressure, owing to friction and to the elevation of sections of the line, is usually not less than 900 lb. on the square inch, and may rise to 1200 or even 1500 lb. The steel tubing employed, therefore, needs to be of exceptional strength, and the screwed couplings to be of special construction. The form of pump is of equal importance, for it must be such as to keep the column of oil in continuous and regular motion, as the concussion which would attend an intermittent flow would quickly have disastrous consequences.

The oil-producing countries of the British Empire are India (Burma and Assam), the West Indies (Trinidad), and Canada, and those in the aggregate furnish only 2 per cent of the total given

Under these conditions the British Government is to be congratulated on having secured the control of the exceptionally prolific oil-fields of Persia. The circumstances in which this important acquisition, which is entitled to rank with Disraeli's purchase of the Suez Canal shares, was rendered possible are worthy of being recorded. Eighteen years ago, the late Mr. W. Knox D'Arcy obtained from the Persian Government a concession for the exploitation of petroleum over the whole of Persia, with the exception of the portion bordering on the Caspian Sea. Successful drilling operations were carried out at Kasr-i-Shirin, on the Turco-Persian frontier, and later the work of development was concentrated in an area about 150 miles from the port of Mohammerah, on the Persian Gulf. The results obtained exceeded the most sanguine anticipations, but the financial burden had become very great, pioneer work of this description in such a country being exceptionally costly, and if Mr. D'Arcy had subordinated his patriotism to his self-interest the concession would have been lost to this country, for at this juncture he received tempting offers from Continental financiers, who were fully alive to the vast importance of it. Supported by Lord Fisher, Mr. Pretyman, the late Sir Gordon Miller and Sir Frederick Black, Mr. D'Arcy declined to entertain in any form the seductive proposals made to him, being determined that at whatever cost to himself the Persian oil-fields should remain under British control, and in 1909, with the powerful co-operation of the Burmah Oil Company and the late Lord Strathcona, the Anglo-Persian Oil Company was formed, with a working capital of £1,200,000. During the succeeding four years the Company carried out a comprehensive scheme of development, under the able guidance of Mr. Charles Greenway, and with the help, up to the time of his death, of Mr. C. W. Wallace. Drilling was actively continued, a pipe-line was laid from the oil-fields to Abadan, on the Persian Gulf, where a refinery was erected, and a large amount of geological exploratory work was done. All this involved a large outlay of capital, and as the Company's cash resources were becoming exhausted there was again a risk of the introduction of foreign control. At this juncture Mr. Greenway was fortunate, with the help of Sir Francis Hopwood (now Lord Southborough), in inducing Mr. Winston Churchill, then First Lord of the Admiralty, to give his personal attention to the position and prospects of the Company, with the result that the British Government purchased a controlling interest in the undertaking for the sum of £2,200,000, and nominated two directors, Lord Inchcape and Admiral Slade, to serve on the Board. This action was taken in the face of considerable opposition, both in the House of Commons and outside, and statements were freely made that the Government had made a disastrous bargain, but at the last Annual General Meeting of the Company Mr. Greenway had the gratification of being able to announce that the profits had already become so large that if the Government wished to dispose of their holding he was prepared to find purchasers for it for not less than six million pounds sterling, and perhaps eight million. Of the vast area of presumably oil-bearing territory which the Company controls only a small part has been brought into active development, but this has proved to be remarkably rich, one of the wells being reported at the meeting to have so far yielded about 1,750,000 tons of oil, and to be still producing as freely as ever, with practically no diminution in the gas-pressure governing the flow.

In the British Isles, as is well known, there is a flourishing industry in the mining and distillation of Scottish shales as a source of mineral oil and ammonia. This industry owes its existence to James Young, of Kelly. In 1847, Young's attention was directed by Playfair to a stream of oil flowing from the top of a coal-working at Alfreton, in Derbyshire. From this oil he succeeded in extracting, on a commercial scale, paraffin-wax, lubricating-oil, and burning-oil. The supply of the raw material being soon exhausted, Young attempted to imitate the natural processes by which he believed the oil to have been produced, by the action of gentle heat on coal, and in 1850 made his invention the subject of his celebrated patent for "obtaining paraffine oil, or an oil containing paraffine, and paraffine from bituminous coals by slow distillation." The process was extensively carried out in the United States, under licence from Young, until crude petroleum was produced in that country in such abundance, and at so low a cost, that the distillation of bituminous minerals became unprofitable.

In this connexion it is interesting to note that in consequence of the approaching exhaustion of the oil-fields of the United States, attention is now being actively given to the utilisation as a source of oil of the immense deposits of bituminous shales known to exist in that country.

The discovery of valuable seams of oil-shale in the early days of the Scottish oil-industry was not characterised by as intense an excitement as prevailed during the development of the older oil-fields of the United States, when sedate business men often suffered from an attack of "oil-fever," but was, nevertheless, not unattended by features of interest. This block of shale, carved into the form of a book, was presented to me twenty years ago by Mr. Stewart S. Robertson, with the following account of its history: "Some forty years ago my father had one morning a visit from a notorious poacher, who undertook, on payment of one sovereign, and the promise of fifty more, to show him something, and never again to poach on our property. The sovereign was paid, and I was deputed to accompany him. On arriving at a certain point where a burn runs between high steep banks, he began picking up bits of what he told me was shale, and showed me a part of the burn which flowed over a smooth black surface of shale. At this time everybody was going shale mad, as Young had just commenced his now well-known works at Addiewell, about 3½ or 4 miles, as the crow flies, from Tarbrax, the place of which I am writing. The poacher received his £50, and never troubled us again. The next day I returned to the spot with a man provided with the necessary tools, and we soon dug a hole two or three feet deep, and took out some pieces of the shale, from one of which the 'book' you have was made. We had numerous offers to lease the land, among them one from Young himself, but my father selected Mr. Fernie as his tenant for 719 acres. Mr. Fernie at once commenced erecting works, but the difficulty of transporting machinery was very great, the site being on a wild moor, and before the works were quite completed Mr. Fernie died. Operations were continued by the executors. The Caledonian Railway made a branch line four miles long on to the property, and eventually the Caledonian Oil Company, Limited, became, and now are, the tenants."

I have mentioned the work carried out by Young on the crude petroleum of Alfreton, and this leads me to refer to the prospects of obtaining free oil in quantity in this country. For many years there was an actual output of petroleum recorded in the General Report and Statistics relating to Mines and Quarries in the United Kingdom, issued by the Home Office. The annual output reached its maximum in 1893, when it amounted to 200 tons, valued at £488. It had fallen to 5 tons, valued at £12, in 1899, and was returned as nil in the following year. There was a recorded production of 8 tons in 1901, and 25 tons in 1902, none in the two succeeding years, 16 tons in 1905, and 10 tons in 1906, the principal locality of production for the latter years being Dumbartonshire. Since 1906 no output has been recorded.

Apart from the production referred to, there have been discoveries of oil in this country from time to time, some of which were of a very doubtful character. An interesting occurrence of free petroleum was brought to my notice in 1892. It took the form of a sudden influx of some hundreds of gallons of light-coloured oil into a well which was the source of the water-supply of an isolated dwelling-house standing on high ground near Shepton Mallet. Another case, which is certainly genuine, is that of the oil-find in a test-boring made for coal at Kelham, near Newark. From this bore-hole, which at a depth of between 2400 and 2500 feet had penetrated a bed of porous sandstone, a flow of characteristic crude petroleum amounting to five or six gallons a day took place. The much-advertised discovery at Ramsey, near Huntingdon, cannot be included in the same category, for the oil, of which a specimen, together with one of the Kelham oil for comparison, is on the table, had unquestionably leaked from an adjacent store.

It may be confidently asserted that in certain parts of Great Britain the geological conditions are consistent with the existence of valuable stores of petroleum, but doubt has been expressed as to whether the drilling operations which the Government has decided to undertake will be attended with success. It is, however, admitted that the only conclusive test is that of the drill. Some months ago Lord Cowdray publicly announced his belief that oil may be found in commercial quantities in Great Britain, and stated that his firm were prepared to spend £500,000 on exploration and development if certain areas were reserved to them.

Even if we should be unsuccessful in finding free oil, we know that we have abundant stores of bituminous minerals from which oil can be obtained by destructive distillation.

For centuries petroleum has been raised from hand-dug wells in Burma, Roumania and Galicia. In the days when King Theebaw reigned in Burma the winning of petroleum by hand-digging in the Yenangyaung district was an important source of revenue. This model, constructed in Burma by native workmen, represents a scene in the oil-field, and it will be observed that women were largely employed in the surface operations. It ought to be liberally sprinkled with crude petroleum to make it realistic. Here, *pace* Rudyard Kipling, East and West have met, in the use of the American kerosene cans for the transport of the oil from the wells. Some of the wells are excavated to a depth of no less than 300 feet, which is the more surprising when it is borne in mind that owing to the presence of petroleum vapour even a young and strong man cannot continue the work for more than about five minutes. The diggers are lowered into and raised from the well by means of the windlass shown, and here is one of the implements which they use in loosening the earth overlying the rock in which the oil occurs. It consists, as will be seen, of a stout wooden staff, shod with iron, the end of the shoe being notched. To break up hard rock when encountered an angular mass of iron is dropped into the well. The detritus is shovelled into baskets and raised to the surface by means of the windlass. The next slide (Fig. 2) represents a scene at the mouth of a well. In the foreground is a digger, with his mouth open, who has just completed his short spell of work in the well and is recovering from the asphyxiating effect of the vapour. On his right is a digger about to take his place, with his eyes bandaged, so that he may be accustomed to the dim light at the bottom of the well and enabled to begin work at once. When the oil-bearing formation has been penetrated the petroleum is brought to the surface in spherical earthen pots, the hauling being done by the women, who run down an inclined plane. The petroleum is carted from the wells in bullock-waggons, one of which forms a prominent feature of the model, to the river-side, where it is poured into the holds of Burmese sailing-boats, of which a model is on the table, and is thus conveyed down the River Irrawadi to Rangoon, where it is refined. The photograph shows a fleet of these boats moored in readiness to receive the cargo.

This primitive system of production has been superseded by the introduction of modern methods of drilling, in which steam-driven machinery is employed. The system most largely adopted in the various oil-fields of the world is that which is known as percussion drilling. The plant consists of a tall pyramidal wooden, or occasionally steel, structure, sometimes as much as 80 feet in height, with an engine-house attached. This model, beautifully carved by Burmese natives, gives a good idea of the plant, notwithstanding the artistic disguise, for it is made to scale. The "string," as it is called, of drilling-tools consists of the "bit" or cutting-chisel, the "auger-stem," the "jars," and the "sinker-bar." Some idea of the nature of the work to be done may be formed when it is stated that the length of a standard deep-drilling Californian string of tools is over 60 feet and the weight 2½ tons. The bit alone, for a bore-hole of 23 inches diameter, of which a full-sized model, kindly made for me by the Oil Well Supply Company, is in front of the lecture-table, has a length of 6 feet and weighs over a ton. The string of tools is suspended by a manilla cable or wire rope from one end of an oscillating beam, the other end of which is connected by a rod with a crank on a shaft driven by the steam-engine. It is thus caused to rise and fall, so that the bit delivers a series of blows on the rock at the bottom of the well, the tools being gradually lowered as the depth increases. The jars may be broadly likened to a pair of flattened links of a chain, and its function is to strike an upward blow when the bit becomes jammed in the rock. From time to time the string of tools is drawn up into the derrick, and the detritus is removed from the borehole by the use of a valved cylinder, termed a sand-

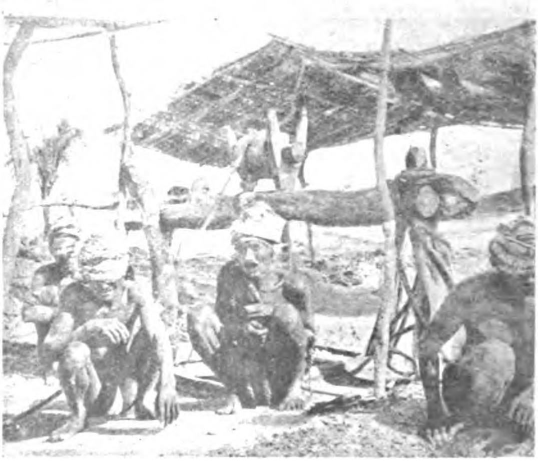

Fig. 2.—Burma. Oil Well.

pump. The next slide (Fig. 3) gives a view of the interior of the lower part of the derrick during drilling operations. The photograph shows the drilling appliances withdrawn from the well, in order that the sand-pump may be introduced.

To exclude water, met with in upper strata, from the well, and to prevent caving, the well is lined with steel artesian tubing, and, as it is usually necessary to reduce the diameter as the boring proceeds, the well may contain several concentric strings of such tubing.

The drilling of petroleum wells has been brought to such perfection that depths of a mile or more may be reached without serious difficulty in a moderate length of time, but the yield of oil needs to be considerable to render drilling to such depths a profitable undertaking. Four years ago there were in the Boryslaw-Tustanowice oil-field of Galicia sixteen wells of a depth of over 5000 feet, and one was then yielding oil from strata which had been reached at 5873 feet, or nearly a mile and a furlong.

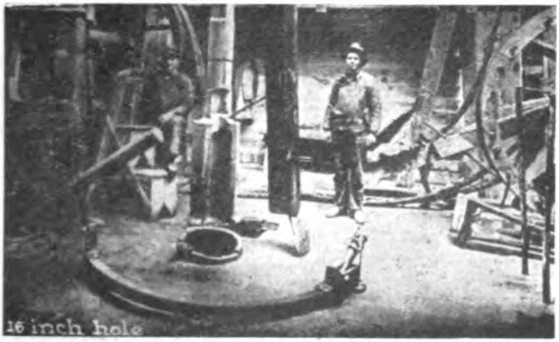

Fig. 3.—Interior of Derrick.

Drilling operations in the tropical jungle are attended with exceptional difficulties of transport. The next two photographs (Fig. 4) are of scenes in the Digboi oil-field of Assam. It will be observed that the patient and intelligent elephant is here employed as a labourer, and that the derricks are thatched to protect the drillers from the sun's rays.

Within recent years, the rotary system of drilling, in which a rapidly-revolving annular cutter, of the type indicated by the example on the lecture-table, is employed, has been largely adopted, with great saving of time when the formation is suitable for its use.

It not infrequently happens that oil is met with on completion of the well under such high pressure, sometimes several hundred pounds on the square inch, that the flow is uncontrollable. Most of us have seen pictorial representations of the famous oil-fountains of Baku, but less is known of similar occurrences elsewhere, which were of an even more remarkable character. The photograph projected on the screen is that of a fountain in the Grozni oil-field in the Northern Caucasus which began to flow in August 1895, and was estimated to have thrown up during the first three days over 4,500,000 gallons, or about 18,500 tons, of oil a day. It flowed continuously, but in gradually diminishing quantity, for fifteen months, quickly destroying the derrick, and afterwards periodically. When I visited the spot in April 1897 there was still an occasional outburst of oil and gas. To save the enormous volume of oil ejected, an army of workmen was employed day and night in throwing a dam across the valley, so as to form a gigantic reservoir, as shown in the next slide. This dam gave way, and a second was constructed below it; a third, still lower down the valley, being afterwards added as a measure of precaution.

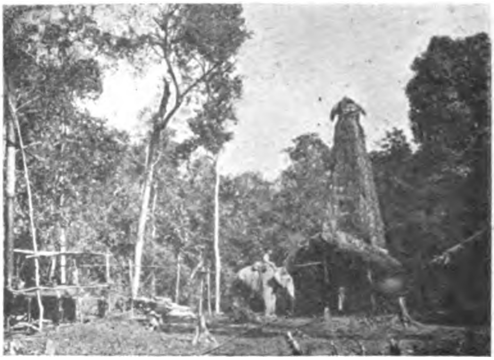

FIG. 4.—ASSAM OIL FIELD.

The huge lake of oil thus formed is shown in the next slide, which represents the scene looking up the valley towards the dam.

Another slide depicts the impressive appearance of a burning fountain on the Rothschild property at Bibi-Eibat. The loss in oil and damage to property during the ten days which elapsed before the fire could be extinguished was estimated at a million roubles.

Probably the most sudden and violent of the outbursts of oil which have been experienced is that which occurred in 1908 on the San Diego property of Messrs. S. Pearson & Son (Lord Cowdray's firm) in Mexico. In the early morning of July 5 in that year oil was struck in a well known as No. 3, at a depth of 1824 feet. The pressure gradually increased, and in fifteen or twenty minutes the ground round the well began to tremble. In various places, some as far distant as 250 feet from the well, fissures appeared, through which oil and gas were emitted. One of these fissures extended under the boilers, and although the fires had been drawn the gas ignited. The flame was immediately communicated to the out-flowing oil, and the well burned for a period of fifty-eight days with an estimated loss of 3,000,000 barrels of oil. A photograph (Fig. 5) of the burning well is now on the screen. For the loan of this, and the two succeeding photographs, I am indebted to Lord Cowdray.

The flames reached a height estimated at 1460 feet, with a maximum breadth of about 480 feet. So brilliant was the light emitted that at 9.40 p.m. on July 8 persons on board a vessel at anchor on the Tamiahua lagoon, a distance of nearly 11 miles from the well, were able to read a newspaper by it. This is the more remarkable when it is considered that the approximate limit of distance at which an object 100 feet high is visible to a spectator at sea-level is a little over 12 miles, so that unless the light from the burning well had been reflected from smoke or cloud, only the upper part of the column of flame could have had illuminating effect at the distance recorded. Besides ejecting the large quantity of oil mentioned, the well yielded a considerable volume of water, estimated to reach at times nearly 1,500,000 barrels daily. This great flow of liquid carried away from the sides of the well solid matter estimated approximately at 2,000,000 tons. On August 31 the flow of oil

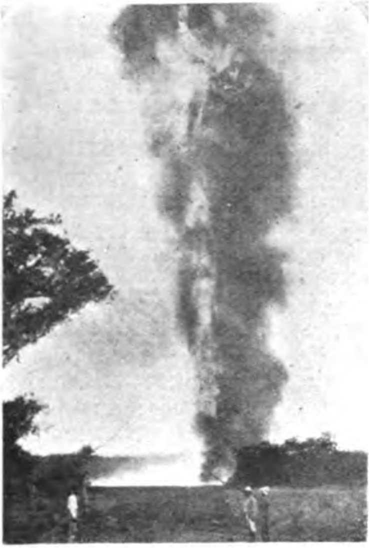

Fig. 5.—Mexico. Burning Well.

temporarily subsided, and it became possible to extinguish the fire by means of sand pumped into the crater with centrifugal pumps. On September 26 the area of the crater was about 15,000 square metres, and on January 28 of the following year, about 117,600 square metres. The two following photographs show Mexican soldiers walking through the fissures caused by the eruption, and the crater as it appeared when the fire was extinguished.

The deposit of ozokerite in Boryslaw, Galicia, is unique, although the mineral occurs in other localities in that country, as well as in Russia and in Utah. The Boryslaw deposit underlies a pear-shaped area, the central and richest part of which is about 50 acres in extent, but this is surrounded by an outer zone of less productive territory, which increases the area of the workable field to about 150 acres. The ozokerite occurs in veins varying from extreme tenuity to many feet in thickness. It is usually plastic, as shown by the specimen on the table, and has evidently been forced up from underlying beds by lateral pressure, through fissures resulting from the local yielding of the marl to the compressive strain. The pressure which still exists is attested by the viscous flow of the ozokerite in the mines, and by the frequent distortion or collapse of the timbering of the galleries. As an illustration of this it is recorded that in one mine the perforation by a miner's pick of a thin impervious stratum of rock forming the floor of a gallery resulted in the gradual appearance of a vertical stalk of ozokerite, which for a long time was replaced when it was removed. This curious appearance of growth gave the name of Asparagus Mine to the working. The mining of ozokerite was formerly carried out in the primitive fashion shown in this locally-constructed model by a large number of proprietors, each of whom had acquired mining rights over a small area which frequently included the dwelling-house. The industry was essentially a domestic one, for it was carried out by the whole family, the sons doing the underground work, whilst the wife and daughters assisted by working the windlass and blowing machine, and the father sat at a table and kept the accounts. It was the practice to sink a shaft to the ozokerite veins, and to drive from the bottom a gallery into the deposit, which was then mined by the pick. The great pressure exerted by the semi-fluid ozokerite necessitated very heavy timbering, and when I visited the locality in 1891 I saw timbers nearly a foot square which had been broken into matchwood by the strain to which they had been subjected. Sometimes the ozokerite suddenly burst into the workings and overwhelmed the miners, and it is reported that men have been unexpectedly raised from the bottom of the shaft to the surface of the ground by such an influx. The horizontal galleries could not be driven further than a few yards on account of the risk, and because of the proximity of the properties. Over the mouth of the well was fixed a windlass carrying a wire rope, to each end of which was attached a bucket used in drawing up the ozokerite, and in lowering and raising the miners. The descent was made by placing one foot in the bucket and holding on to the rope, the other being used in fending the bucket off the sides of the shaft. Owing to the subsidence and lateral movement of the ground, due to the removal of the ozokerite, the shafts did not long remain vertical, and the descent was not a pleasant experience. The miner wore a safety-belt, to which a rope was attached. Much inflammable gas was met with in the galleries, and safety-lamps were necessarily used. Ventilation was unsatisfactorily effected by means of a fan-blower. Within easy reach of the miner was a cord attached to a bell at the mouth of the shaft, by means of which he could summon assistance; but notwithstanding the provision of the bell and safety-belt deaths by suffocation were not uncommon. The first effect of the inhalation of the gas produced a kind of intoxication, which some of the miners appeared to find enjoyable. It is not surprising that work of this character, often conducted with insufficient capital, inadequate appliances, and imperfect organisation, should have been attended with much unnecessary loss of life, especially when the somewhat reckless character of those engaged in it is taken into account. It was stated in 1871 that among the 2000 underground workers then employed there were usually from 200 to 300 accidents yearly, nearly all of which were fatal, and that in some years the number of deaths was as great as 1000. The ozokerite candles which many of us can remember were made from ceresin, a product obtained by subjecting the ozokerite to destructive distillation in a current of superheated steam. These candles were specially suited for use in tropical countries, owing to the high melting-point of the material. Purified ozokerite has also been largely employed as a substitute for or adulterant of beeswax, which it closely resembles in physical properties, especially in the manufacture of church candles, in cases where the employment of paraffin-wax is prohibited by ecclesiastical law.

Crude petroleum varies greatly in character, as is shown by the representative collection of specimens from various parts of the world which is on the lecture-table, some descriptions being of pale colour and highly mobile, whilst others are almost black and viscid. The specific gravity appears to range from 0·771 to 1·06.

As regards its chemical composition, petroleum consists essentially of carbon and hydrogen, together with oxygen, and varying amounts of nitrogen and sulphur.

Pennsylvanian petroleum consists chiefly of a large number of hydrocarbons of the paraffin series, whilst naphthenes or polymethylenes are the predominant constituents of Russian petroleum. In some descriptions of crude petroleum, notably those of the Dutch East Indies, Persia, and Burma, aromatic hydrocarbons are largely present.

These paraffins and naphthenes are very accommodating, in the sense that they readily lend themselves to conversion, by dissociation, or "cracking," as it is termed, into other compounds of carbon and hydrogen, of lower boiling-point or higher volatility, which are so largely in demand at the present time in the form of motor-spirit. The conversion occurs when the oil is distilled under pressure or is brought into contact with highly-heated surfaces. The chemical changes which occur in these circumstances and the constitution of the products were investigated by Thorpe and Young many years ago. In 1888, I was privileged to be associated with the Fullerian Professor in experimental work which involved the construction of suitable apparatus for carrying out the process on a practical scale, and it was found that in order to obtain the best results it was necessary to effect the condensation of the vapour also under pressure. The process devised at that time is essentially the same as that which is now very largely carried out in the United States, with the object of augmenting the inadequate supplies of motor-spirit normally obtainable from the crude oil by fractional distillation. By carrying this treatment further it is possible to obtain aromatic hydrocarbons, and I have on the lecture-table specimens of the high explosive trinitrotoluene, or T.N.T., as well as benzene and dye-products, thus obtained from petroleum containing no aromatic hydrocarbons.

The refining of petroleum, briefly described, consists in the classification of the hydrocarbons, by means of fractional distillation, into the various commercial products, including motor-spirit, lamp-oil, heavy fuel-oil, lubricating oils of various grades, and solid paraffin, and the subsequent purification of these products. The simplest form of fractionating apparatus comprises a still, of which a model is on the table, and a condenser consisting of piping, which may be coiled or straight. Of such an arrangement a photograph (Fig. 6) is now projected on the screen. It represents what is probably the most primitive petroleum refinery in the world, situated at Sherkat, in Mesopotamia, near the Tigris. The stills and condensers are plainly shown, and here again we see, in the foreground, the ubiquitous American kerosene can.

This model represents a petroleum refinery of modern type, with dephlegmators interposed between the stills and the condensers, to effect the separation of the more readily condensable hydrocarbons.

In the early years of the industry petroleum products were largely stored, and wholly distributed, in barrels and tin cases, but the system now universally adopted is that of storage and transport in bulk. The type of tank employed for the storage of the oil is shown by the model. These tanks are constructed of steel plates, and are often as much as 90 to 100 feet in diameter by 30 to 40 feet in height. The largest holds about 2,000,000 gallons. For the transport of the oil specially constructed tank steamers, which unfortunately are the objects of unwelcome attention on the part of the enemy's submarines, are used. These vessels, of one of which a sectional model is before you, are with few exceptions driven by steam-engines, and as a rule the steam is raised by the combustion of oil fuel, but within recent years some have been built with internal-combustion engines, with resulting economy in fuel consumption. Of the later additions to the large fleet of such vessels, some are of great size, carrying as much as 15,000 tons of oil, but the more usual cargo capacity is from 5000 to 10,000 tons.

The evolution of the tank steamer occupied many years. As far back as 1878, Ludwig Nobel, the Swedish pioneer in the Russian petroleum industry, constructed the first of these vessels employed on the Caspian Sea in the transport of petroleum from Baku to the mouth of the Volga. At an earlier date, viz. between 1869 and 1872, a wooden ship fitted with 59 small tanks, with an aggregate capacity of 794 tons, had been employed in the transport of crude petroleum from the United States to Europe, and in the latter year the first steamship was built for the carrying of oil in bulk across the Atlantic, but was never used for the purpose. It was not, in fact, until 1885, seven years after Nobel had demonstrated the success of the system, that the first tank steamship employed in the Transatlantic service was built.

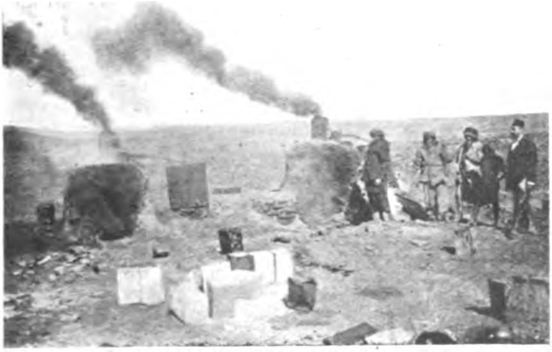

FIG. 6.—MESOPOTAMIA. REFINERY.

There was at first diversity of opinion among naval architects and shipbuilders as to whether the oil tanks should be formed of the "skin" of the ship, as shown in this model, or should be independent, but the former construction is now always adopted. The two essential features are the expansion trunks, which ensure that the main tanks shall always remain full, and the coffer-dam, which isolates the tanks from the machinery space. The expansion trunks extend upwards from the tanks, like the neck of a bottle, and although of relatively small area hold enough oil to compensate for the reduction in volume of the contents of the tanks resulting from decrease in temperature, and conversely afford adequate storage room for oil expelled owing to increase in temperature. If the main tanks were not kept completely full, dangerous oscillation of the cargo would take place when the ship was rolling, and as an additional precaution the tanks are fitted with an amidship fore-and-aft bulkhead, as shown. The coffer-dam, at the after-end of the series of tanks, is formed by a double bulkhead, and it may be filled with water so as to prevent any oil which may leak from the adjacent tank from finding its way aft. At first the conservatism of shipbuilders, or perhaps the artistic instincts of the nautical man, led to the engines being placed in the usual position, amidships. This, however, necessitated the provision of a second coffer-dam, as well as an oil-tight tunnel from the screw-shaft, extending through the after-tanks, and the propelling machinery is now usually placed aft, as shown. The pumps for discharging the cargo are an important part of the equipment. The pump-room is here shown below deck immediately forward of the machinery space. The diameter of the piping connected with the tanks, and the capacity of the pumps, are now such that a cargo of 10,000 tons can be discharged in twelve hours. The more modern vessels are fitted with mechanical ventilating appliances by means of which air can be forced into or drawn from any oil-compartment, and an accumulation of vapour prevented.

Besides these tank-ships, tank-barges of the description known as "dumb," therefore requiring to be towed, are largely used, and are often of great size. We are all familiar with the appearance of the petroleum tank-wagons on our railways and in our streets. By means of these, and the ocean-transport already described, a very large proportion of the products made in the principal petroleum refineries of the world reaches the wholesale or retail distributor, and in many cases the consumer, without being placed in packages of any description.

But for the disabilities imposed on me by the Official Secrets Act, the stringent regulations made under the Defence of the Realm Act, and the Censor, the subject could have been presented in a more entertaining manner; but I hope I have been able to deal with it in such a way as to justify the title which I had the temerity to select.

[B. R.]

GENERAL MONTHLY MEETING,

Monday, July 1st, 1918.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Thomas Glover

was elected a Member.

His Grace The Duke of Northumberland was elected President; and Sir Thomas Wrightson, Bart., was elected a Visitor.

The Chairman announced that the Managers, at their Meeting held this day, had appointed Professor Sir James Dewar, F.R.S., Fullerian Professor of Chemistry.

The Chairman announced the decease of the following Members since the last Meeting:—

The Earl of Rosse,
Dr. H. G. Plimmer.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Pusa: Memoirs of the Department of Agriculture, Entomological Series, 1918, No. 78. 8vo.
Board of Scientific Advice for India: Annual Report, 1916–17. 8vo

American Chemical Society—Journal of Industrial and Engineering Chemistry for June 1918. 8vo.

American Journal of Physiology—Vol. XLVI. No. 2, May. 8vo. 1918.

Boletin Mensual de Estadistica Municipal de la Ciudad de Buenos Aires—No. 1, March 1918. 4to.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXV. No. 8, June 1918. 8vo.

British Association for the Advancement of Science—Report, 1917 18. 8vo.

British Astronomical Association—Journal, Vol. XXVIII. No. 7, May 1918. 8vo.

British Museum, Trustees of—Catalogue of English Coins—Norman Kings, 1916. 8vo.
Ancient Greek Inscription in the British Museum, Part 4, Section 2, 1916. Folio.

Chemical Industry, Society of—Journal, Vol. XXXVII. No. 2, June 1918. 8vo.

Chemical Society—Journal for June 1918. 8vo.

Craig, Walter Lennox (the Author)—Sterling Decimal Coinage, May 1918. 8vo.

Editors—Aeronautical Journal for April 1918. 8vo.
Athenæum for June 1918. 4to.
Author for June 1918. 8vo.

Editors—continued.
Chemical News for June 1918. 4to.
Chemist and Druggist for June 1918. 8vo.
Church Gazette for June 1918. 8vo.
Dyer and Calico Printer for June 1918. 4to.
Electrical Times for June 1918. 4to.
Electricity for June 1918. 8vo.
Engineering for June 1918. fol.
General Electric Review for June 1918. 8vo.
Horological Journal for June 1918. 8vo.
Journal of the British Dental Association for June 1918. 8vo.
Junior Mechanics for June 1918. 8vo.
Law Journal for June 1918. 8vo.
Model Engineer for June 1918. 8vo.
Nature for June 1918. 4to.
New Church Magazine for June 1918. 8vo.
Science Abstracts for May 1918. 8vo.
Wireless World for June 1918. 8vo.
Zoophilist for June 1918. 8vo.

Florence, Biblioteca Nazionale Centrale—Bolletino for March-April 1918. 8vo.
Franklin Institute—Journal, May 1918. 8vo.
Helvetica Chimica Acta—Vol. I. April 1918. 8vo.
Institute of Bankers—Journal, Vol. XXXIX. Part 6, June 1918.
Institution of Electrical Engineers—Journal, June 1918, Vol. LVI. No. 275. 4to.
Kyoto Imperial University—Vol. II. No. 6, Oct. 1917; Vol. III. No. 1, Dec. 1917.
London County Council—Gazette for June 1918. 4to.
Meteorological Office—Weekly Weather Reports for June 1918. 4to.
Daily Readings for April 1918. 4to.
Geophysical Journal, June 1917. 4to.
Meteorological Service of Canada—Annual Report, 1915.
Pharmaceutical Society of Great Britain—Journal for June 1918. 8vo.
Photographic Society, Royal—Journal, June 1918. 8vo.
Quekett Microscopical Club—Journal, April 1918. 8vo.
Röntgen Society—Journal, Vol. XIV. No. 55, April 1918.
Royal Colonial Institute—United Empire, Vol. IX. No. 6. 8vo. 1918.
Royal Observatory, Greenwich—Report of the Astronomer-Royal, June 1, 1918. 4to.
Royal Sanitary Institute—Journal, Vol. XXXIX. June 1918. 8vo.
Royal Society of Arts—Journal for June 1918. 8vo.
Royal Society of London—Proceedings: A, Vol. XCIV. No. A662; B, Vol. XC. No. B627. 8vo.
Sanderson, The Right Hon. Lord—The Province of Burma, Vols. I.-II. 8vo. 1907.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXIV. No. 6. June 1918. 8vo.
Society of Antiquaries—Proceedings, Second Series, Vol. XXIX. Nov. 1916–June 1917. 8vo.
Archæologia, or Miscellaneous Tracts Relating to Antiquity, 1916–17. 4to.
Tôhoku Imperial University, Sendai, Japan—Science Reports, First Series, Vol. VI. No. 5, June 1918. 8vo.
Mathematical Journal, April 1918.
United States Department of Agriculture—Journal of Agricultural Research, Vol. XIII. April-May 1918, Nos. 3-6. 8vo.
Experiment Station Record, May 1918, Vol. XXXVIII. Nos. 3-4; Vol. XXXVII. Index No. 8vo.
Washington, National Academy of Sciences—Proceedings, Vol. IV. No. 15. May 1918.

GENERAL MONTHLY MEETING,

Monday, November 4, 1918.

HIS GRACE THE DUKE OF NORTHUMBERLAND in the Chair.

The Special Thanks of the Members were returned to Richard Pearce, Esq., F.G.S., for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperature.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Bulletin of Agricultural Research Institute, Pusa, No. 77. 1918. 8vo.

Academie des Sciences de Lisbonne—Journal de Sciencias Matematicas, Fisicas e Naturias, Tome I. Nos. 1–2. 1917. 8vo; No. 3. 1918. 8vo.
  Urosemiologia Clinica. 8vo. 1918.
  Depois de Fememoto, Vol. I. 8vo. 1916.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali, Vol. XXVII. 1° Semestre, Fasc. 3-4, 8–10; April-June, Fasc. 12. 1918. 8vo.

Admiralty, Lords Commissioners of the—The Nautical Almanac for Year 1921. 8vo. 1918.

American Chemical Society—Abstracts for June-Sept. 1918. 8vo.
  Journal of Industrial and Engineering Chemistry for July-Sept. 1918. 4to.

American Geographical Society—Geographical Review, May-Sept. 1918. 8vo.

American Journal of Physiology—Vol. XLVI. No. 5, Aug. 1918; Vol. XLVII. No. 1, Sept. 1918. 8vo.

American Philosophical Society—Proceedings, Vol. LVII. Nos. 2–4. 1918. 8vo.

Asiatic Society, Royal—Journal for July-Oct. 8vo. 1918.

Astronomical Society, Royal—Monthly Notices, Vol. LXXVIII. No. 9, May 1918. 8vo.
  List of Fellows and Associates for June, 1918. 8vo.

Australia, Commonwealth of—Advisory Council of Science and Industry, No. 1. 1918. 8vo.
  Agricultural Research in Australia, Bulletin No. 7. 1918. 8vo.
  South Australian School of Mines and Industries: Annual Report for 1917. 8vo. 1918.

Bankers, Institute of—Journal, Vol. XXXIX. Part 7, October 1918. 8vo.

Boletin Mensual de Estadistica, Municipal de la Cuidad de Buenos Aires—Nos. 3–4, March-June 1918. 4to.
  Boletin de la Direccion General de Estadistica, Departmento Provincial del Trabajo. Prov. de Buenos Aires, No. 198. 8vo.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXV. Nos. 10–12, Aug.-Oct. 1918. 4to.

British Astronomical Association—Journal, Vol. XXVIII. No. 8, June 1918. 8vo.
  Annual Report. 1918. 8vo.

British Dental Association—Journal, Vol. XXXIX. Nos. 18–20. 8vo. 1918.
Canada, Department of Mines—Geological Survey: Memoirs, Parts 1-5. 8vo. 1918.
Museum Bulletin, No. 27. 8vo. 1918.
Mines Branch: Analyses of Canadian Fuels. 8vo. 1918.
Bulletin No. 20, Part II. 1918. 8vo.
Report on Building and Ornamental Stones of Canada, Vol. V. No 452. 8vo. 1917.
Mineral Springs of Canada. 8vo. 1918.
Annual Report, Mineral Production of Canada, 1916. 8vo. 1918.
Report of Clay Resources in Southern Saskatchewan, No. 468. 8vo. 1918.
Geological Survey, No. 86, Quebec. 8vo. 1918; No. 77, Onaping Map-area. 8vo. 1917.
Canada, Royal Society of—Transactions,
Carnegie Endowment for National Peace—Year-Book, 1918. 8vo.
Carnegie Institute of Washington—Reprints, Nos. 137-141. 1917. 8vo; Nos. 52–56. 1918. 8vo.
Contributions from Mount Wilson Solar Observatory, Nos. 142–146. 1917. 8vo.
Chemical Industry, Society of—Journal, July–Oct. 8vo. 1918.
Chemical Society—Journal for June Oct. 1918. 8vo.
List of Officers and Fellows. 1918. 8vo.
Chemistry, Institute of Great Britain and Ireland—Proceedings, Part 3. 1918. 8vo.
Civil Engineers, Institution of—List of Members. 1918. 8vo.
Crerar, The John, Library—Annual Report. 1917. 8vo.
Croydon Public Libraries—Bi-Monthly Magazine, Readers' Index, No. 5. 8vo. 1918.
Editors—Aeronautical Journal for July, 1918. 8vo.
Athenæum for June–Aug. 1918. 4to.
Author for June–Oct. 1918. 8vo
Chemist and Druggist for July–Oct. 1918. 8vo.
Church Gazttte for July–Oct. 1918. 8vo.
Concrete for July–Oct. 1918. 8vo.
Dyer and Calico Printer for July–Oct. 1918. 4to.
Electrical Times for July–Oct 1918. 4to.
Engineering for July–Oct. 1918. fol.
Freight and Metal World, Vol. XXXIII. No. 1669, Aug. 1918. 4to.
General Electric Review for July–Oct. 1918. 8vo.
Horological Journal for Aug.–Oct. 1918. 8vo.
Illuminating Engineer for May–July 1918. 8vo.
Journal of Physical Chemistry for June–Oct. 1918. 8vo.
Junior Mechanics for July-Oct. 1918. 8vo.
Law Journal for July–Oct. 1918. 8vo.
London University Gazette for July–Sept. 1918. 4to.
Model Engineer for July–Oct. 1918. 8vo.
Musical Times for July–Oct. 1918. 8vo.
Nature for July–Oct. 1918. 4to.
New Church Magazine for July–Oct. 1918. 8vo.
Physical Review for July–Oct. 1918. 8vo.
Science Abstracts for June–Aug. 1918. 8vo.
Wireless World for July–Oct. 1918. 8vo.
Zoophilist for Aug.–Oct. 1918. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LVI. No. 276. 4to. 1918.
Estudio de la Ciencias—Serie Matematico Fisica, Vol. I. No. 29, March 1917. 8vo.
Faraday Society—Transactions, Vol. XIII. Part 3. 8vo. 1918.
Firenze, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani, No. 207, May–June, 1918. 8vo.

Formosa, Government of—Flora of Formosa, Vol. VII. March 1918. 4to.
Franklin Institute—Journal, June-Oct. 1918. 8vo.
Geographical Society, Royal—Journal for July-Oct. 1918. 8vo.
Supplement to No. 1, June 1918. 8vo.
Geological Society of London—Abstracts of Proceedings.
Quarterly Journal, Vol. LXXIII. 8vo. 1918.
Geological Survey of Great Britain—Memoirs, 1917. 8vo.
Glasgow, Royal Philosophical Society—Proceedings, Vol. XLVIII. 1917-18.
8vo. 1918.
Hadfield, Sir Robert—Abstract of Co-ordination of Scientific Publications
(pamphlet). 8vo. 1918.
Helvetica Chimica Acta—Vol. I. 8vo. 1918.
Horticultural Society, Royal—Journal, Vol. XLII. Parts 1-2, 1916-17. 8vo.
1918.
Imperial College of Science and Technology, London — Calendar, Session
1918-19. 8vo. 1918.
Iron and Steel Institute—Journal, Vol. XCVII. No. 1. 1918. 8vo.
Johns Hopkins University, Baltimore—University Circular, New Series, 1917,
Nos. 293-300. 8vo. 1917; New Series, 1918, Nos. 301-304. 8vo. 1918.
University Studies, Series 35, Nos. 2-3. 8vo. 1917; Series 36, Nos. 1-3.
8vo. 1918.
Jordan, W. Leighton, Esq. (the Author)—The Squaring of the Circle
(pamphlet). 8vo. 1918.
Kansas University—Science Bulletin, Vol. X. No. 1, Jan. 1917. 8vo.
Kyōto Imperial University—Memoirs of College of Science, Vol. III. No. 2,
Feb. 1918. 8vo.
Life-Boat Institution, Royal National—Journal for Aug. 1918, Vol. XXIII.
No. 265. 8vo. 1918.
London County Council—Gazette for July-Nov. 1918. 4to.
London Society—Journal for July-Sept. 1918. 8vo.
London University—Gazette for July 1918. 4to.
Manchester Literary and Philosophical Society—Memoirs and Proceedings,
1917-18, Vol. LXII. Part 1. 1918. 8vo.
Manchester, School of Technology—Journal of the, Vol. IX. 4to. 1918.
Mechanical Engineers, Institution of—Proceedings, Jan.-May 1918. 8vo.
Mental Science—Journal of, Vol. LXIV. No. 266, July 1918. 8vo.
Meteorological Office—Monthly Weather Reports for May-July 1918. 4to.
Weekly Weather Reports for April-Aug. 1918. 4to.
Daily Readings for May-June 1918. 4to.
Geophysical Journal—Daily Values for June-July 1918.
Geophysical Memoirs, No. 12. 4to. 1918.
Monthly Weather Report for Year 1917, Part 2. 4to.
Weekly Weather Report, 1917, Appendix 2. 4to.
Hourly Values from Autographic Records, Circular Nos. 28-29. 8vo. 1918.
Thirteenth Annual Report, 1917-18. 8vo. 1918.
Metropolitan Asylums Board—Annual Report for Year 1917. 8vo. 1918.
Ministry of Munitions—Munitions Inventions Department, Physical and
Chemical Data of Nitrogen Compounds, April 1918. 4to.
Monaco, Musée Océanographique—Bulletin, Nos. 340-343. 8vo. 1918.
Montenegrin Committee for National Union—Bulletin, Nos. 4-5, Sept. 1918.
8vo.
Myers, Lieut.-Col. Charles S. (the Author)—Present-Day Applications of
Psychology. 8vo. 1918.
National Physical Laboratory—Report for Year 1917-18. 4to. 1918.
New Jersey, State of—Department of Conservation and Development, Report
for the Year ending Oct. 31, 1917. 8vo.
New York Academy of Sciences—Annals, Vol. XXVII. pp. 215-243. 8vo.
Aug. 1917.

New York, Society for Experimental Biology—Proceedings, Vol. XV. No. 8. 1918. 8vo.

New Zealand, High Commissioner for—Patent Office Journal, Vol. VII. Nos. 9-15. 1918. 4to.
Census, Oct. 1916. 4to.
Statistics for Year 1916, Vols. III.-IV. 4to.
Official Year Book, 1917. 8vo.

North British Association of Gas Managers—William Young Memorial Lecture. By John W. Cobb (pamphlet). 8vo. 1918.

Numismatic Society, Royal—Numismatic Chronicle, Part I. 4th Series, Vol. XVIII. 1918. 8vo.

Peru, Guerpo de Ingenieros de Minas—Boletins, Nos. 87, 89. 8vo. 1918.

Pharmaceutical Society of Great Britain—Journals for July-Oct. 1918. 8vo.

Philadelphia Academy of Natural Sciences—Proceedings, Vol. LXIX. Parts 2, 4, April-Sept. 1917; Vol. LXX. Part 1, 1918. 8vo.

Photographic Society, Royal—Journal, June 1918. 8vo.

Physical Society of London—Proceedings, Vol. XXX. Parts 4-5. 1918. 8vo.

Pusa, Agricultural Research Institute—Journal, Vol. XIII. Part 3, July 1918. 8vo.

Real Academia de Ciencias—Revista de la Exactas, Fisicas y Naturales de Madrid, Tome XV. Nos. 10-12. 1917. 8vo; Tome XVI. Nos. 1-5. 1917. 8vo.

Rockfeller Foundation, The—Review, 1917. 8vo.

Rockfeller Institute for Medical Research—Reprints, Vol. XXVIII. 8vo. 1918.

Royal College of Surgeons of England—Calendar. 1918. 8vo.

Royal Colonial Institute—United Empire, July-Sept. 1918. 8vo.

Royal Engineers' Institute—Journal, Vol. XXVIII. No. 1, July 1918. 8vo; Nos. 3-5, Sept.-Nov. 1918. 8vo.

Royal Society of Arts—Journals for July-Oct. 1918. 8vo.

Royal Society of Edinburgh—Transactions, Vol. LII. Part 1, Session 1917-18. 4to. 1918.
Proceedings, 1917-18, Vol. XXXVIII. Part 2. 8vo. 1918.

Royal Society of London—Proceedings, A, Vol. XCV. Nos. A665-A666, July 1918. 8vo; B, Vol. XC. July 1918. 8vo.

Royal Statistical Society—Journal, Vol. LXXXI. Part 3, May; Part 4, July. 1918. 8vo.

Royal United Service Institution—Journal of, Vol. LXIII. No. 451, Aug. 1918. 8vo.

Scientific Societies, Conjoint Board of—Preliminary Report of Water Power Committee, July 1918. 8vo.

Scottish Geographical Society, Royal—Scottish Geographical Magazine. Journal, Vol. XXXIV. No. 7, July 1918. 8vo.

Smithsonian Institution—Miscellaneous Collections, Vol. LXIX. No. 6, June 1918. 8vo.
Atmospheric Scattering of Light, Vol. LXIX. No. 3, May, 1918. 8vo.
Annual Report of the Smithsonian Institution.

Societa degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. VII. Disp. 4-6. 1918. 4to.

Society for Experimental Biology and Medicine—Proceedings, Vol. XV. No. 7, April 1918. 8vo.

South Africa, Union of—Department of Agriculture, Bulletin No. 2. 1918. 8vo.

South African Association—Report for the Advancement of Science, Kimberley, 8vo.

Spielmann, Sir Isidore—Germany's Impending Doom. (An Open Letter to Herr Maximilian Harden.) No. 2. 8vo. 1918.

Stonyhurst College Observatory—Results of Meteorological, Magnetical and Seismological Observations, 1917.

Tôhoku Imperial University, Sendai, Japan—Science Reports, 2nd Series, Vol. III. No. 2: Vol. IV. No. 3. 1918. 4to.

Tôhoku Mathematical Journal—June-Aug. 1918.

Toronto, University of—Transactions of Royal Canadian Institute, 1st Series, Vol. VII. No. 1, July; No. 26, Nov. 1917, Vol. XI. Part 2. 8vo.

Studies, Nos. 3, 14–16, 58. 8vo. 1918

United States Department of Agriculture—Journal of Agricultural Research, May-Sept. 1918. 8vo. Index, Vol. XI.

Experiment Station Record, Vol. XXVIII. Nos. 5-6, April-June; Vol. XXXVIII. Nos. 7, 9; Vol. XXXIX. No. 1. 1918. 8vo.

United States Department of Commerce—United States Coast and Geodetic Survey, 1915-16. 4to. 1918.

Serial No. 61: Special Publication, No. 42, for 1916. 4to. 1917.

Serial No. 84, Magnetic Observations, Sitka, Alaska, 1915–16. 4to. 1918.

Serial No. 86, Results of Observations made in Honolulu Magnetic Observatory, 1915–16. 4to. 1918.

Serial No. 88, Terrestrial Magnetism, Magnetic Observations for 1917. 8vo. 1918.

United States Patent Office—Official Gazette, Vol. CCL. Nos. 1–4; Vol. CCLII. No. 1. 1918. 8vo.

Upsala, l'Université d' l'Observatoire Météorologique—Bulletin Mensuel, Vol. XLIX. 1917. 4to.

Uber die Gegenstrahlung der Atmosphare. By Anders Angstrom (Upsala). 1917. 4to.

Arkiv. for Matematik, Astronomi och Fysik, Band 13, Nos. 7-8. 1918. 8vo.

Upsala, Royal Society of—Nova Acta, Ser. 4, Vol. V. No. 1. 4to. 1918.

Washington, National Academy of Sciences—Proceedings, Vol. IV. Nos. 7-8. July-Aug. 1918. 8vo.

Western Society of Engineers—Journal, Vol. XXIII. No. 1, Jan.; No. 2, Feb. 1918. 8vo.

Zanichelli, Nicola—Casa Editrice, Bologna, I Fenomeni Elettro Atomici sotto l' Agione del Magnetismo. 8vo. 1918.

GENERAL MONTHLY MEETING,

Monday, December 2, 1918.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D., F.R.S.,
Treasurer and Vice-President, in the Chair.

The Secretary announced the decease of Sir Charles Norris Nicholson on November 29, 1918.

The following Resolution of condolence was submitted to the Members and unanimously passed:—

The Members of the Royal Institution of Great Britain desire to record their sense of the loss the Institution has sustained through the decease of Sir Charles Norris Nicholson, Bart., M.P., M.A. (Cantab.), LL.B., Vice-Chairman of the London War Pensions Committee.

Sir Charles Norris Nicholson was a member of the Institution for twenty-eight years, and as a Manager took an active interest in the business affairs of the Institution, and the objects of the Foundation in the promotion and diffusion of Scientific knowledge.

Resolved, That the Managers desire to express, on behalf of the Members of the Royal Institution, their most sincere sympathy with Lady Nicholson and the family in their bereavement.

Report from the Managers, December 2, 1918 :—

The Managers reported, That they had received a Present from Mrs. Edward Pollock of the Portraits of her Father and Mother, the late Dr. and Mrs. Warren de la Rue; and a Donation of 26*l*. 5*s*. from Dr. Dundas Grant to the Fund for the Promotion of Experimental Research at Low Temperatures.

Resolved, That the special Thanks of the Members be returned to Mrs. Edward Pollock for her Present and to Dr. Dundas Grant for his Donation.

The Chairman declared in the terms of the Bye-Laws, Chapter 4, Article 2, That there was a vacancy in the Office of Manager through the decease of Sir Charles Norris Nicholson, Bart., M.P., and at the next General Meeting on February 3 the Vacancy will be filled in accordance with the said Bye-Law.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Bulletin of Agricultural Research Institute, Pusa, Nos. 81-82. 1918. 8vo.
Agricultural Journal of India, Vol. XIII. Part 4. 1918. 8vo.
Geological Survey of India, Records, Vol XLIX. Part 1. 1918. 8vo.
Accademia dei Lincei Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XVIII. 1 Semestre, Fasc. 1. 1918. 8vo.
American Chemical Society—Abstracts for Oct.-Nov. 1918. 8vo.
Journal for Nov. 1918. 8vo.
Journal of Industrial and Engineering Chemistry for Oct.-Nov. 1918. 4to.
American Geographical Society—Geographical Review, Oct. 1918. 8vo.
Index to Bulletin, 1852-1915. 8vo. 1918.
American Philosophical Society—Proceedings, Vol. LVII. No. 5. 1918. 8vo.
American Red Cross—Trench Fever Report of Medical Research Committee, 1918. 8vo.
Australia, Commonwealth of—Advisory Council of Science and Industry, Report for 1918. 4to. 1918.
Agricultural Research in Australia, Bulletin No. 8. 1918. 8vo.
Bankers, Institute of—Journal, Vol. XXXIX. Parts 8 and 9. October 1918. 8vo.
British Dental Association—Journal, Vol. XXXIX. Nos. 1-21. 8vo. 1918.
Cambridge Philosophical Society—Transactions, Vol. XXII. Nos. 12-14. 1918. 4to.
Chemical Industry, Society of—Journal, Oct.-Nov. 8vo. 1918.
Chemical Society—Journal for Oct.-Nov. 1918. 8vo.
Editors—Aeronautical Journal for Aug. 1918. 8vo.
Athenæum for Nov.-Dec. 1918. 4to.
Author for Nov.-Dec. 1918. 8vo.
Chemist and Druggist for Nov.-Dec. 1918. 8vo.
Church Gazette for Nov.-Dec. 1918. 8vo.
Concrete for Nov.-Dec. 1918. 8vo.
Dyer and Calico Printer for Nov.-Dec. 1918. 4to.
Electrical Times for Nov.-Dec. 1918. 4to.
Engineering for Nov.-Dec. 1918. fol.

Editors—continued.
  Freight and Metal World, Nov.-Dec. 1918. 4to.
  General Electric Review for Nov.-Dec. 1918. 8vo.
  Horological Journal for Nov.-Dec. 1918. 8vo.
  Illuminating Engineer for Nov.-Dec. 1918. 8vo.
  Journal of Physical Chemistry for Nov. 1918. 8vo.
  Junior Mechanics for Nov.-Dec. 1918. 8vo.
  Law Journal for Nov.-Dec. 1918. 8vo.
  London University Gazette for Nov.-Dec. 1918. 4to.
  Model Engineer for Nov.-Dec. 1918. 8vo.
  Musical Times for Nov.-Dec. 1918. 8vo.
  Nature for Nov.-Dec. 1918. 4to.
  New Church Magazine for Nov.-Dec. 1918. 8vo.
  Physical Review for Nov. 1918. 8vo.
  Science Abstracts for Sept. 1918. 8vo.
  Wireless World for Dec. 1918. 8vo.
  Zoophilist for Oct.-Nov. 1918. 8vo.
Firenze, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani, No. 208, July-Aug. 1918. 8vo.
Franklin Institute—Journal, Nov.-Dec. 1918. 8vo.
Geographical Society, Royal—Journal for Nov. 1918. 8vo.
Geological Society of London—Abstracts of Proceedings, No. 1028. 8vo. 1918.
Hadfield, Sir Robert—The Occlusion of Gases by Metals. (Faraday Society Discussion). (pamphlet). 8vo. 1918.
Harrison-Austin (Editor)—English Review, Nov. 1918. 8vo.
Helvetica Chimica Acta—Vol. I., Part 5. 8vo. 1918.
Iron and Steel Institute—Carnegie Scholarship Memoirs, Vol. IX. 8vo. 1918.
Leiden—Physical Lab. Communications from, Nos. 150-151. 8vo. 1918.
Life-Boat Institution, Royal National—Journal for Aug. 1918, Vol. XXIII. No. 266. 8vo. 1918.
Linnean Society—Proceedings, Oct. 1918. 8vo.
London County Council—Gazette for Nov.-Dec. 1918. 4to.
London University—Gazette for Sept. 1918. 4to.
Meteorological Office—Daily Readings for July-Sept. 1918. 4to.
  Geophysical Journal—Daily Values for Aug.-Nov. 1918.
Monaco, Musée Océanographique—Bulletin, Nos. 344-347. 8vo. 1918.
Oxford University Press—The Periodical, Vol. VI. No. 98, Oct. 1918. 8vo.
Pharmaceutical Society of Great Britain—Journal for Nov. 1918. 8vo.
Photographic Society, Royal—Journal, Nov. 1918. 8vo.
Pusa, Agricultural Research Institute—Journal, Vol. XIII. Part 4. 1918. 8vo.
Royal Engineers' Institute—Journal, Vol. XXVIII. No. 6. 8vo. 1918.
Royal Irish Academy—Proceedings, Vol. XXXIV., A, Part 4; B, Parts 4-6; C, Parts 8-9. 1918. 8vo.
Royal Society of Arts—Journal for Nov. 1918. 8vo.
Royal Society of London—Proceedings, A, Vol. XCV. No. 667, 1918. 8vo.; Vol. XXXV. Part 1, 1918. 8vo.
Saleeby, Dr. C. W. (the Author)—(Pamphlet.) Race Renewal: The Ideal of a Ministry of Health. 1918. 8vo.
Sanitary Institute, Royal—Journal, Vol. XXXIX., No. 2. 1918. 8vo.
Scottish Geographical Society, Royal—Scottish Geographical Magazine.
  Journal, Vol. XXXIV. No. 11, Nov. 1918 8vo.
Smithsonian Institution—Report of the U.S. National Museum, 1917. 8vo. 1918.
South Africa, Union of—Official Year Book, 1910-16. 1917. 8vo.
United States Department of Agriculture—Journal of Agricultural Research, Oct. 1918. 8vo.
Yorkshire Archæological Society—Journal, Vol. XXV. No. 97. 8vo. 1918.

WEEKLY EVENING MEETING,

Friday, March 22, 1918.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. D.C.L. F.R.S.,
Hon. Prof. Natural Philosophy, R.I., in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.Sc. Pres. R.S.,
Professor of Natural Philosophy, R.I., Master of Trinity.

Radiation from System of Electrons.

[NO ABSTRACT.]

[A footnote in the Philosophical Magazine, Vol. XLI. p. 510, "On the Structure of the Molecule and Chemical Combination," by Sir J. J. Thomson, states, "Many of the results in this paper were given by me in my Lectures at the Royal Institution between 1914 and 1919, but owing to the pressure of other duties I have not hitherto had leisure to prepare them for publication."]

WEEKLY EVENING MEETING,

Friday, May 10, 1918.

THE HON. R. CLERE PARSONS, M.A. M.Inst.C.E.,
Vice-President, in the Chair.

PROFESSOR F. GOWLAND HOPKINS, M.B. D.Sc. F.R.S.

The Scientific Study of Human Nutrition.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, January 18, 1918.

His Grace the Duke of Northumberland, P.C. K.G.
LL.D. F.R.S., President, in the Chair.

Professor Sir James Dewar, M.A. LL.D. D.Sc. F.R.S. M.R.I.,
Fullerian Professor of Chemistry.

Studies on Liquid Films.

Some problems presented by liquid films were discussed in the Discourse of 1917.* These studies have been continued and further developed in the restricted time left for research after meeting the demands of Government Departments; but such conditions imply a certain amount of discontinuity which cannot but leave its mark on the inquiry.

BUBBLES FOUR FEET IN DIAMETER.

In order to increase the size of bubbles above those described a year ago (46 cm. in diameter), advantage was taken of a cool cement-lined cellar (some 600 cubic feet capacity) which was so fitted that the nozzle and blowing tube were passed through the door and controlled from outside. All apertures and cracks were filled with glycerined cotton wool. The atmosphere of the cellar was several times cleaned and renewed by pumping in purified air, but no permanent purity could be maintained. In less than two days the Tyndall cone would reappear. Large sheets and trays smeared with glycerin were of no assistance in reducing the mist, so that although bubbles up to four feet in diameter were obtained, they did not last above a few hours.

PLANE BLACK FILMS 2500 SQ. CM. IN AREA.

At the Discourse given a year ago a stoppered vessel was shown which contained a plane black film 19 cm. in diameter; the film was then one week old, and has remained unchanged to this date.

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* "Soap Bubbles of Long Duration," Proc. Roy. Inst., Vol. XXII. p. 179.

For the purpose of extending the study to larger films, the cylindrical globes that were used last year for the examination of 40 cm. bubbles were employed. Plane films 56½ cm., or nearly two feet in diameter, were thereby obtained which lasted for 40 days, and even more, after becoming entirely black. Fig. 1 shows a globe fitted for this purpose. About half a litre of soap solution having been run in, and the end of the tube A dipped into it, a bubble, blown by pure air, was started and gradually expanded, as shown by the various stages of the diagram, until it became a plane horizontal film about the middle of the vessel. Some idea of the appearance of the film in the globe can be gathered from Fig. 2, reproduced from a photograph taken with a white screen placed behind and a little above the film, thus reflecting diffused light.

The globe and its fittings must be scrupulously clean, and the interior free from all traces of floating matter when tested by the Tyndall cone of light. By careful manipulation the soap solution was made to flow all over the inside, while the tube A was also wetted by a stream of soap solution from a dropping funnel F fitting tightly in the rubber cork used to close the neck of the globe. The tube A can slide freely up and down in the support tube B, to which was sealed the bulbed outlet E. The upper part of A could, on occasion, be fitted with a dropping funnel C, sealed to the vertical portion, while still connected to the bulbed nozzle D, by which pure air was admitted. The bulbs D, E were loosely packed with cotton wool moistened with glycerin. C was only used when subsequent additions had to be made to the soap solution in the globe.

After the plane film was obtained in the right position midway in the globe, A was raised until its lower end was a few inches below the film. The nozzles D and E were open, but were provided with soda-lime guard tubes, so that the spaces above and below the film were equally open to the atmosphere, though guarded from contamination. This ensured that the fluctuations of the atmosphere were equalised throughout both portions of the relatively large space of the globe; otherwise the film was not likely to last long because of the vertical displacements that would occur. In one case a barometric alteration of 1 cm. in 20 hours caused the film to move through 5 mm. After the globe has drained for a day or two, there is not enough liquid left on the walls to keep the film sufficiently lubricated to allow of such oscillations. The only liquid available is in the tiny channel—the Gibbs layer—round the periphery of the film. This channel is roughly triangular in section, with the base of the triangle on the glass walls, and the concave sides converging to the film which extends out beyond the apex of the triangle. In a very well-drained film the base of this triangle may not be more than 0·1 mm., so that the sectional area of the channel is about $\frac{1}{20000}$th of a sq. cm. The total liquid available round such a film 56 cm. diameter is, therefore, only about 1 cgm. It is obvious that

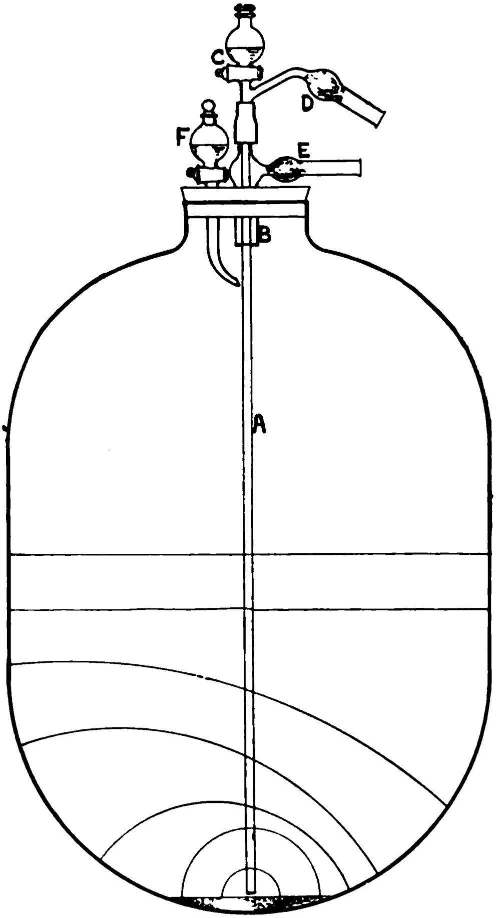


Fig. 1.

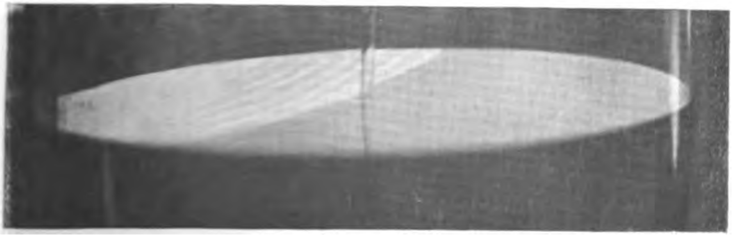


Fig. 2.

long before such complete drainage as this is reached any movement of the film occurring over a relatively dry surface will quickly exhaust the liquid in the channel, and the film will be ruptured. However, when the proper precautions were taken, the displacements due to atmospheric variations did not amount to more than 1 or 2 mm. for weeks together.

Until they become completely black very striking colour effects can be seen in these large horizontal films. They have of course only a small gradient of thickness, so that the bands of colour are drawn out into large areas. During this period convection in the films is most easily seen, because of the brilliant contrasts afforded by the moving portions. Should there be much fluctuation of temperature, the complete development to the black state may be arrested, the last part of the coloured area being kept in intermittent circulation for several days. Superfluous liquid draining down outside the central tube will produce a similar result by becoming spread out on the black film surface as a coloured clot round the tube. When this clot grows sufficiently it will draw off slowly, and pass down the radius of greatest inclination of the film. It formed on one occasion an ellipse (1 cm. by 1½ cm.) which took 35 seconds to traverse the last one-fourth of its path. On approaching the periphery where the film curves up slightly into contact with the glass, the clot flattened out and curled into the most intricate convolutions, finally sweeping round close to the edge, a writhing mass of coloured streaks, many of which became roving stars on the black film. The whole coloured area was finally drawn into the Gibbs ring, from which frequent escapes of small drops of liquid took place.

A favourable condition for longevity is a quiet temperature rather below 10° C. Sudden alterations of 3° or 4° C. are most likely to burst the films by provoking too rapid convection therein, as is evidenced by very rapidly moving streams of silvery stars drawn from the Gibbs ring.

The vibrations which occurred during the operation of the air compressors in the Laboratory did not affect the final stability of the black films. A stationary circular wave was produced having an amplitude of over 1 mm. vertically all round a circle midway between the central tube and the walls of the globe. This motion continued the whole time the machines were working, but the films always survived.

Instead of having one large plane black film, a mesh-work of black films can be obtained completely filling the globe. To produce this, the lower end of the blowing tube A (Fig. 1) is inserted slightly below the surface of the soap solution. The air current being regulated suitably to the diameter of the tube A, a steady succession of uniform bubbles is produced. The surface of the glass having previously been well moistened, the issuing bubbles conse-

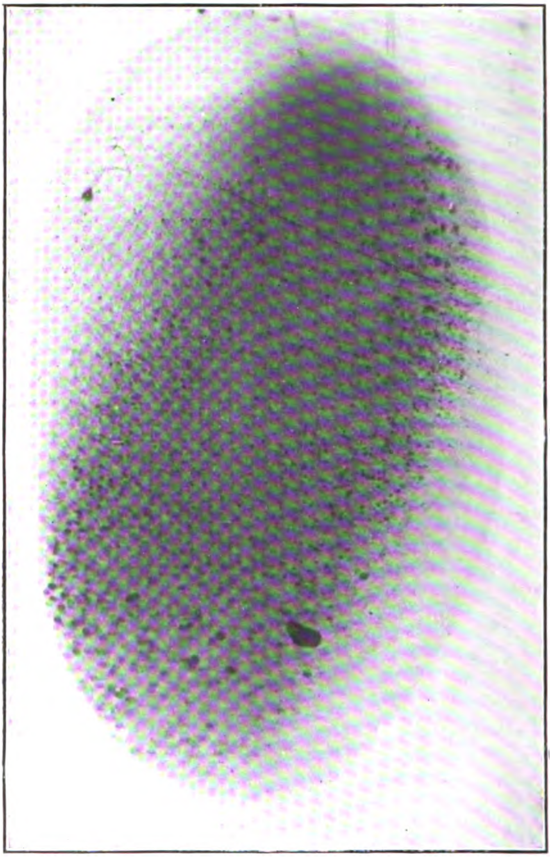


FIG. 3*a*.

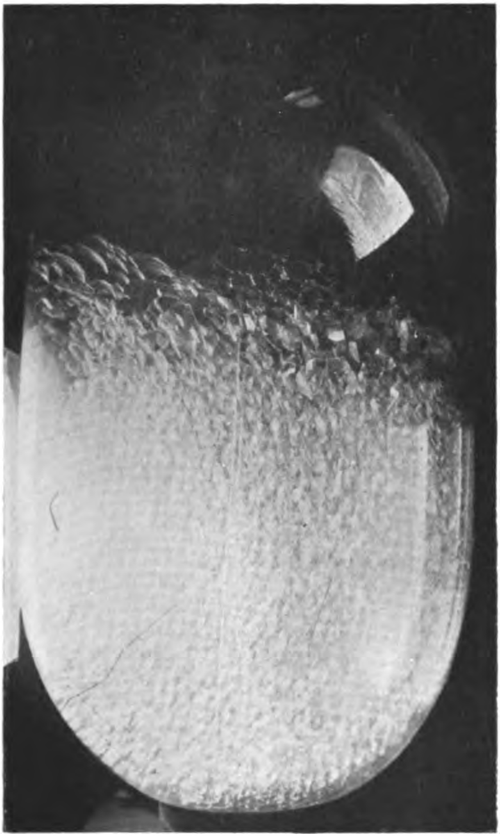


FIG. 3*b*.

quently encounter little resistance, and develope rapidly, linking up into a glittering lustrous froth which increases until the globe is full. Excess liquid drains away very rapidly, the black stage being reached in about an hour. The beauty of the mass can be seen properly only when a beam of light is flashed upon it in continually changing directions, the different inclinations of the internal plane films giving rise to a continuous series of brilliant scintillations (Fig. 3*a*). The same appearance is perhaps still more remarkable when the mesh-work is lit up by diffused light from rays thrown across the observer's line of vision (Fig. 3*b*). A black background is used in both cases.

The internal walls of the various cells being plane polygons, the pressure of the air in these cells is the same for all, and is regulated according to the curvature of the cell-walls on the outer boundary. As transference of the enmeshed air from the mass can take place only through the curved boundaries of the outer layer of bubbles, contraction of the mass from this cause is relatively slow. In one case it required a fortnight for the initial volume to be reduced to one half.

To obtain the best result the blowing tube A should be withdrawn when the mesh-work has been completed, as it exerts some protecting power over the bubbles that closely surround it, thus causing an annular depression farther out, and tending to destroy the mass.

Columns and Chains of Bubbles.

For the purpose of studying bubble clusters and film complexes, instead of single bubbles and plane films, an arrangement of the nozzle was employed, by which it was possible to get a succession of bubbles of any required volume, linked together, in one operation. The apparatus employed is shown in Fig. 4. A is a stoppered reservoir of soap solution from which a capillary tube passes down the air-supply tube D, B, to about 1 cm. above a constriction K. After passing K the lower end of the air-supply tube is, for convenience, fitted by a ground joint to an enlarged nozzle C, securely held in position by the rubber cork E. The stopper A being opened at regulated intervals of time, a series of drops falls on the constriction K, which are immediately blown into films by the constant air current entering through the bulb D (protected as before by lightly packed glycerined cotton wool). Hence a series of bubbles is produced at the nozzle C,

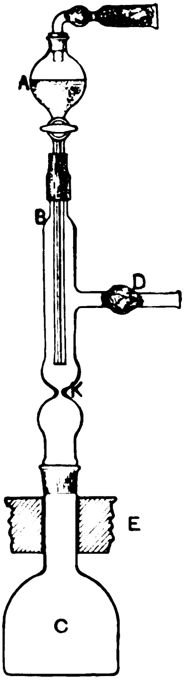


Fig. 4.

which may have equal or different volumes as desired, and may be further controlled so as to present themselves either as a vertical column or as a cluster.

The shape of the cluster obtained depends on the dimensions of the nozzle, the speed of the air current, and the relation between the mass of the drop and the size of the constriction. If the drop be too large or the air current too slow, extra films result from the accumulation of superfluous liquid; the same thing happens with an elongated constriction. On the other hand, too rapid a current may simply spray the liquid over the interior of the nozzle. The cleanest working is obtained from a short neck blown out above and below in a spherical shape as shown in the figure at K.

A succession of equal bubbles may either form a regular cluster on the mouth of the nozzle, or a vertical chain or column of spherical segments united by plane circular films. A cluster will usually be formed when the bubbles are less in diameter than the nozzle, and a chain or column when the bubbles are larger. For the manipulation of these complexes an air-tight, cubical, plate-glass chamber was made (Fig. 5), on a frame of aluminium alloy (edge, 50 cm.). The glass plate for the top was 1 cm. thick; holes were cut in it for the rubber corks that held the nozzles and supported the movable glass rods which carried the different small pieces of apparatus used to catch and control the chains while being formed.

For the manipulation of the longer columns the glass cylinder shown in Fig. 6 was used. It was 3 feet long and 6 inches in diameter; its ends made air-tight joints with two plates, the upper of which held the blowing nozzle, while the lower held a support ring. Suitable exit stopcock and soda-lime guard tube were also fitted. The support ring was sealed to the top of a vertical rod that could slide up and down in an air-tight joint in the bottom plate. The cylinder was fixed on a shelf in the middle of a massive wooden stand, and a hole was cut in the shelf to allow free movement to the sliding rod. When a to-and-fro motion was given to the support rod, the flexibility of the column was readily seen; transverse waves appeared to pass up the whole length, each segment moving on the one adjacent as if attached by a very free universal joint. By drawing the rod down a considerable extension could be obtained; contrariwise by raising it a bulging or spiral formation would result. If this be carried further and the surface be sufficiently moist, the column will sag out into contact with the glass walls and form a chain of oval segments. In a wider vessel, however, the segments can in this way be linked up successively to form regular clusters.

A beautiful exhibition of multiple scintillating coloured reflections is obtained when a beam from an arc lamp below is directed up through the column while the support rod is moved. An even more complete illumination is secured by using a white lined hood above, in which is hidden a 200-watt lamp, with two white wings of stiff

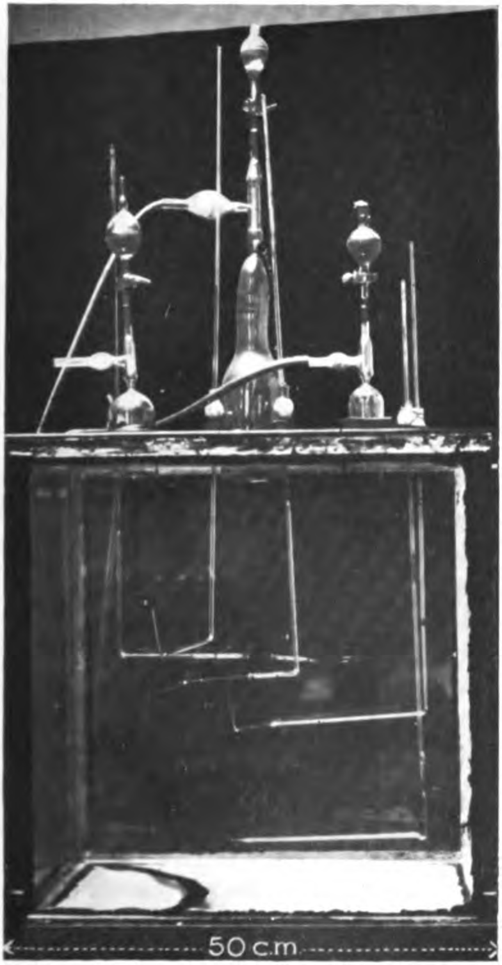


Fig. 5.

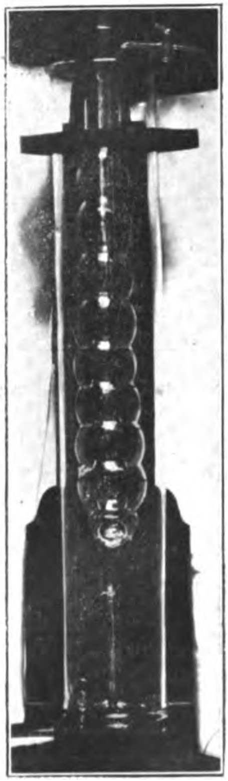


Fig. 6.

card fixed one on each side of the central black stand. These side screens alone are sufficient for this purpose if the whole apparatus stands in a good light. A striking contrast is obtained when the column is drained to blackness and illuminated by a beam from below, if an occasional drop of soap solution is allowed to trickle down through the nozzle. Before this is done very little is seen except the bright point reflections from the curved surfaces; but, as the falling drops arrive in rapid succession at the planes between the adjacent segments, the thin circular channels flash up into sparkling silvery light from the internal reflections of the accumulated liquid. Fig. 7 gives some idea of the appearance.

The following particulars show how some of the results were obtained:—

(*a*) Nozzle, 3 cm. diameter; constriction, 1·15 mm. bore; drop, 18 mgms.: air current approximately 500 c.c. a minute; drop interval, 5 seconds; giving a volume of 42 c.c. for each segment. While they were being blown the free end was caught on one of the movable glass rings already mentioned, by means of which fifteen bubbles were steadily drawn out into a flexible catenary with its ends 13 cm. apart (Fig. 8).

(*b*) Nozzle, 3·4 cm. diameter; air current, 565 c.c. per minute; drop interval, 7½ seconds; giving a volume of about 70 c.c. for each segment. A column of eight of these bubbles reached from the rim of the nozzle to the wet floor of the cube, where contact was made. The same nozzle could give a chain of nine bubbles of three times this volume.

(*c*) Same nozzle; air current, 305 c.c. per minute; drop interval, 9½ seconds; giving a volume of about 48 c.c. for each segment. A column of ten of these bubbles was held on a fairly straight axis inclined at about 50° to the horizontal after they were drained free of excess liquid.

Some of these chains are liable to break with excess liquid (Fig. 8). This can be largely obviated by using a percentage of hydrogen in the air current, provided an atmosphere of pure air is maintained in the glass cube. The buoyancy of the bubbles is thereby increased for a time sufficient to allow the column to be manipulated. When the chain has once been secured any further accumulation of liquid must be continually removed. For this purpose a fluted glass rod some 2 mm. thick and 3 or 4 cm. long was very effective. (It was made by drawing out a bundle of seven small glass rods in the blow-pipe, and was utilised by attachment to one of the movable bent glass rods passing through the top of the cubical box.)

When very large bubble segments are required it is safer to interrupt the air current between two adjacent segments, while a few extra drops of liquid are run in to remoisten the glass surfaces. In this way bubbles of over 1 litre were obtained on a nozzle 5 cm.

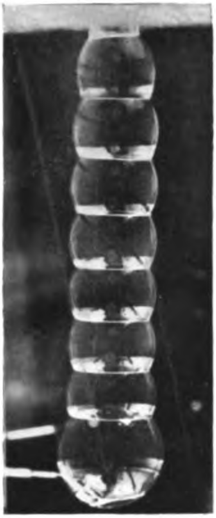


FIG. 7.

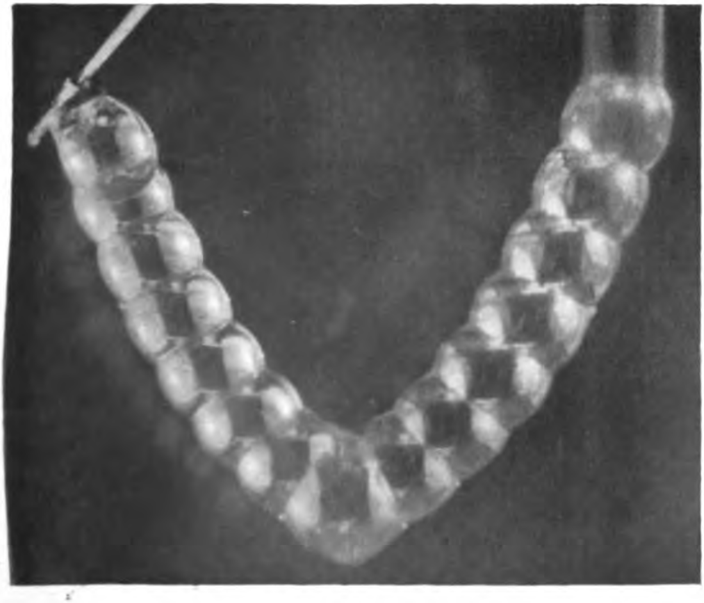


FIG. 8.

in diameter, with a constriction of 1 mm. bore, and a 25 mgm. drop. Four of these linked together and forming a chain inclined at about 45° to the vertical were photographed on a Lumière autochrome plate, which, in addition to the coloured contours, showed clearly the brilliant bands on the plane films between the equal segments. To obtain these photographs the cubical glass vessel was placed in full sunlight. The top and two opposite sides were covered with thin tissue paper to give uniform diffuse illumination. Black paper formed the background, while the front was left clear for exposure

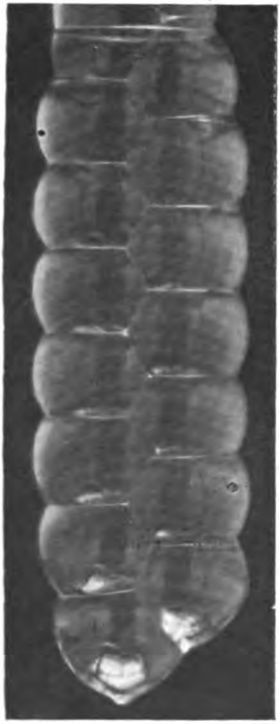


FIG. 9.

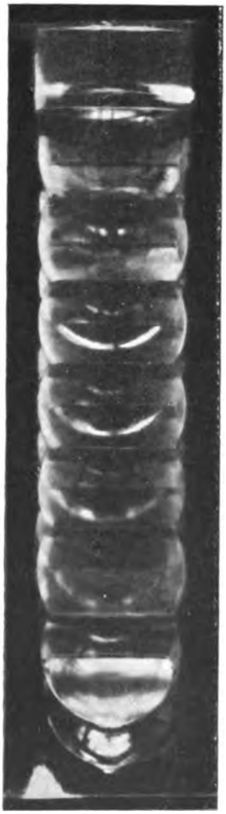


FIG. 10.

to the camera. On a quarter-plate an exposure of 40 to 60 seconds was necessary. A 9 by 7 inch Ross portrait lens was employed at F. 11. Colour photographs of several of the columns have been obtained in this way.

MULTIPLE COLUMNS.

Regular formations of greater complexity were obtained when the bubble elements were reduced in size so that their diameters were less than that of the nozzle. Thus, for example, two columns which coalesced into one are shown in two perpendicular views in Figs. 9 and 10 (see also Fig. 24). The horizontal plane films between the successive elements of both columns were linked by a "staircase" of films, inclined right and left at 120° to the horizontal films; every bubble thus fitted into two bubbles of the adjacent half

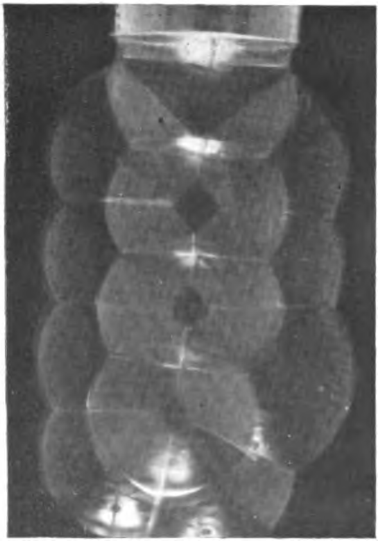


FIG. 11.

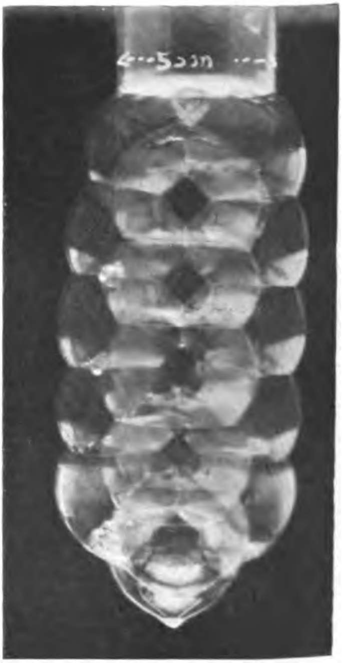


FIG. 12.

column, the horizontal junction films being segments of circles, while the inclined films took the form of rectangles with their short sides slightly convex. The data for this formation were: Nozzle, 5 cm. diameter; air current, 130 c.c. per minute, with an interval of 15 seconds between successive drops. Each element of the column thus had a volume of 82½ c.c.

Three columns will also coalesce from top to bottom, twining round each other like the spiral strands of a rope. The plane films linking the bubble segments are then truncated sectors of circles.

A complex of four interpenetrating vertical columns is shown in Figs. 11 and 12. This may be considered as built up by a succession of horizontal pairs of bubbles, each pair being at right angles to the adjacent pairs above and below. The symmetrical pairs of horizontal plane films which link them are circular quadrants with their points in contact, and of course are alternately at right angles.

Between each member of the alternate horizontal pairs of bubbles there are vertical films which are diamond-shaped, and joined at their apices with their planes alternately at right angles along the axis of the cluster. The resulting appearance is very regular. One example of this formation was produced by a nozzle of 5 cm. diameter, with an air current of 100 c.c. a minute, and an interval of 4 to 5 seconds between the drops.

A still more complex but perfectly regular quadruple column has the simple string of diamond-shaped films replaced by alternate hexagons and squares of which only the hexagons are vertical. An isometric view of this part of the arrangement is shown in Fig. 13. The common edges of adjacent squares and hexagons are horizontal, and alternate squares are equally inclined in opposite directions to the axis of the complex column. The complete configuration is shown in Figs. 14 and 15 (two perpendicular views); the outlines are further displayed in Fig. 16.

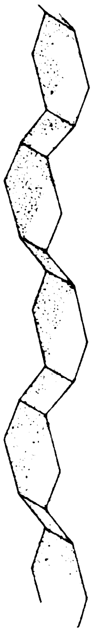


FIG. 13.

Multiple columns of a higher order were obtained by lessening the time-interval between the drops. The excess liquid thus supplied tended to distort the cluster. This was avoided by employing a long nozzle within which the network of bubbles was formed and supported until the excess liquid drained away. The cluster was then expanded clear of the nozzle, and remained stable and undistorted. As many as seven or more interlaced vertical columns were obtained in this manner. The centre bubbles of the cluster were of course completely enclosed, and hence had no curved surfaces, but formed instead a chain of film-enclosed cells. From the outermost facets of these there radiated a film structure of pyramidal frustums bounded externally by curved bubble films. Two forms of these faceted "cells" are shown in Fig. 17; some have the regularity of a well-cut gem, and exhibit great brilliance in a concentrated light. Some forms frequently occurring have (*a*) parallel regular hexagons top and bottom with pentagonal facets; others have (*b*) irregular heptagons at the top and bottom: the facets being then either pentagons alone or alternate hexagons and quadrilaterals, or even all three together. An interesting example occurred when the quadruple column with alternate diamond-shaped planes distributed along the axis (Fig. 11) was disengaged from its nozzle and allowed to fall on the surface of the soap solution 15 cm. below. The cluster had already drained to complete blackness, and was therefore so light that at first it rebounded several cms., finally floating on the surface for some time, delicately poised like a nautilus.

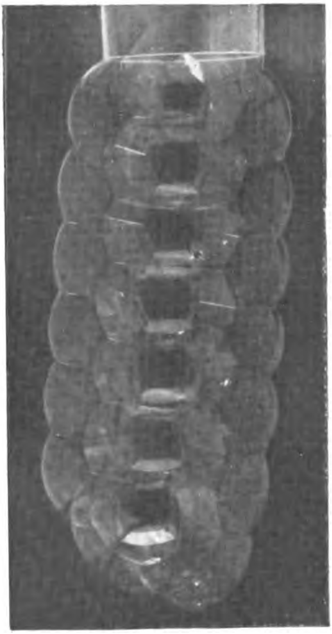


FIG. 14.

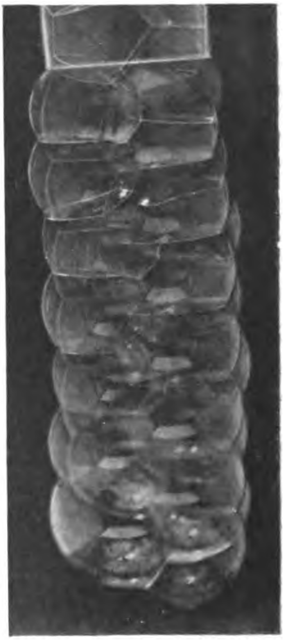


FIG. 15.

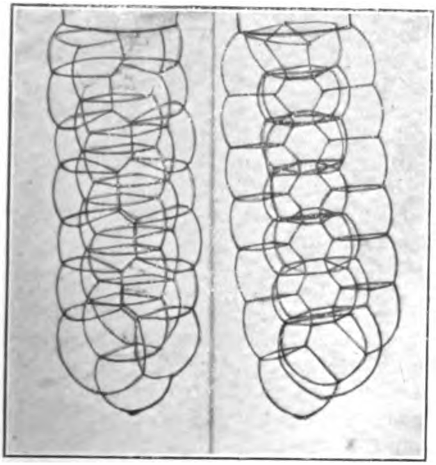


Fig. 16.

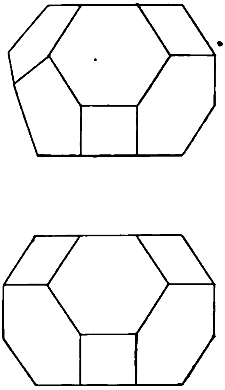


Fig. 17.

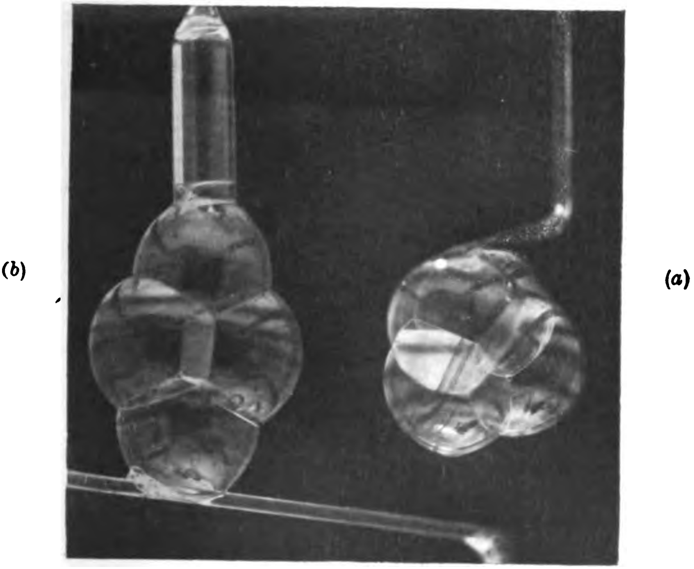


Fig. 18.

However, it soon coalesced with the liquid surface, forming a half dodecahedron in the centre surrounded by a symmetrical ring of six half bubbles all resting on the liquid, and three whole bubbles superposed regularly above these, each of the nine external bubbles springing from one facet of the central "cell." The completed structure, of which this was the half, would thus have consisted of twelve pentagonal pyramidal frustums round a dodecahedron, the bases of the pyramids being the outer curved bubble surfaces. Such a complex was obtained separately, but it is diffcult to show the configuration by means of a photograph.

SYMMETRICAL CLUSTERS OF BUBBLES.

Equal-sized bubbles are readily built up into well-defined groups that have many interesting properties. Their spherical contours, interlaced by a network of reflecting films, give them a graceful symmetry. Up to an aggregate of eight the growth is fairly simple. Each successive bubble springs instantaneously into its place with the appearance of being partly absorbed, though of course retaining its volume practically unchanged. Two bubbles thus coalesce into two spherical segments joined by a circular plane film. When a third bubble is introduced between these two the spherical boundaries are further reduced, and are spaced out between three equally inclined plane films, which are segments of circles in contact round the axis of symmetry.

With four bubbles there are two possible arrangements; the more stable form has the fourth bubble resting symmetrically on the group of three, so that the four segments are equally spaced on the frame of a regular tetrahedron. Six plane films in the shape of circle sectors are thus produced by the four segments coalescing round the centroid of the tetrahedron (Fig. 18*a*). But if the fourth bubble be introduced between any two of the three-group instead of at the junction of all three, then the form shown in Fig. 18*b* is obtained, which has a central plane film shaped like a rectangle with its short sides slightly curved, and two pairs of equally inclined planes springing from its long sides. These five planes constitute a frame linking the four bubble segments together.

In Fig. 19 these frames outlined in wire are shown as they appear (*a*) for the group of three: and (*b*), (*c*) for the two forms of the group of four; while Fig. 20 (outer set) gives the sections through the groups to show the inclination between the plane films, and the trace of the outer contours.

Only one arrangement of the five-group has been obtained. It is formed when two additional segments interpenetrate, one at each end of the axis of the three-group, the three upper plane films thus formed, meeting in one point of this axis, and the three lower plane films meeting in another point of the same axis.

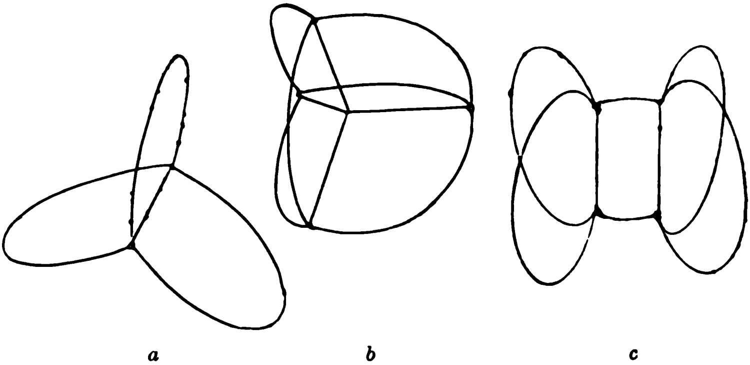


Fig. 19.

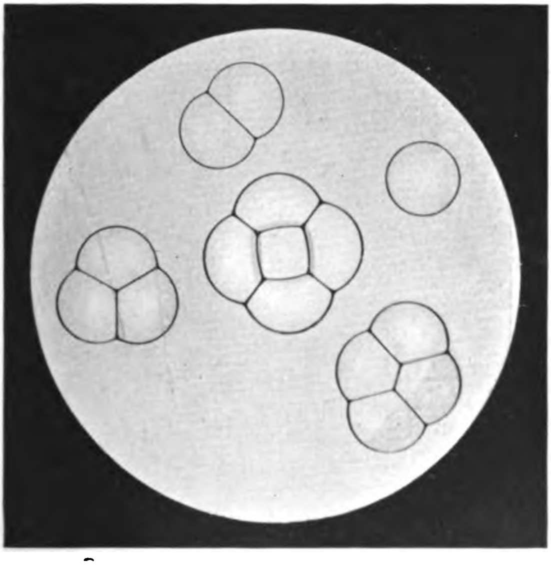


Fig. 20.

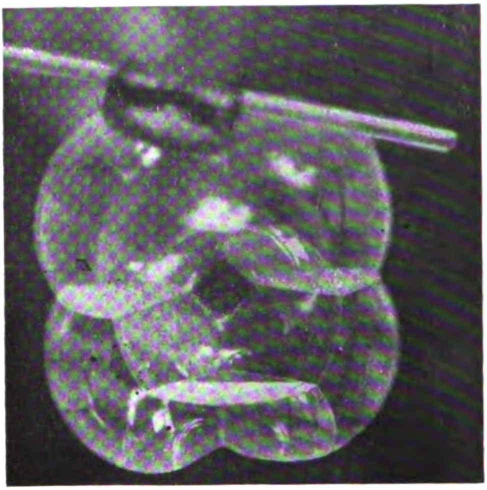


FIG. 21*a*.

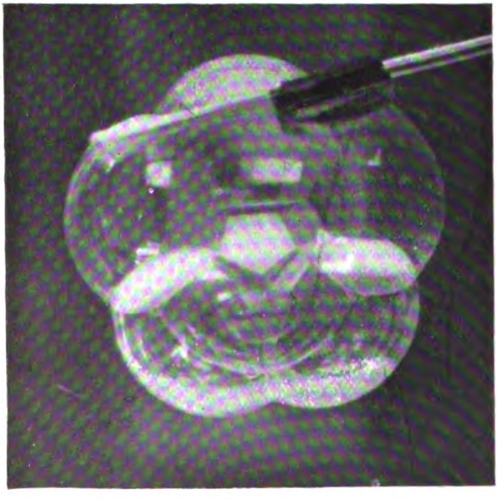


FIG. 21*b*.

The groups of six, seven and eight all have the same type of symmetry—namely, at the centre is a small plane polygon, from the sides of which there radiates a ring of bubble segments, equally spaced, with two others on an axis at right-angles to the central plane. The six-group therefore has a ring of four segments round a square central plane, the two others being on opposite sides of this central plane. The two interlocked bubble segments thus take the form of two truncated square pyramids in contact with each other on the central square plane, having their bases bulged out on the spherical contour, and the four other segments built in round the faces of the truncated pyramids (Fig. 21*a*). The section of this is shown in the central group of Fig. 20. The groups of seven and eight are similarly formed, except that the square central plane is replaced by a pentagon and hexagon respectively. In all cases the polygon edges are slightly curved. Fig. 21*b* is a photograph of the seven-group with pentagon centre. The wire frames for this group and the eight-group with hexagon centre are shown in Fig. 22, while sections through the two are given in Fig. 23.

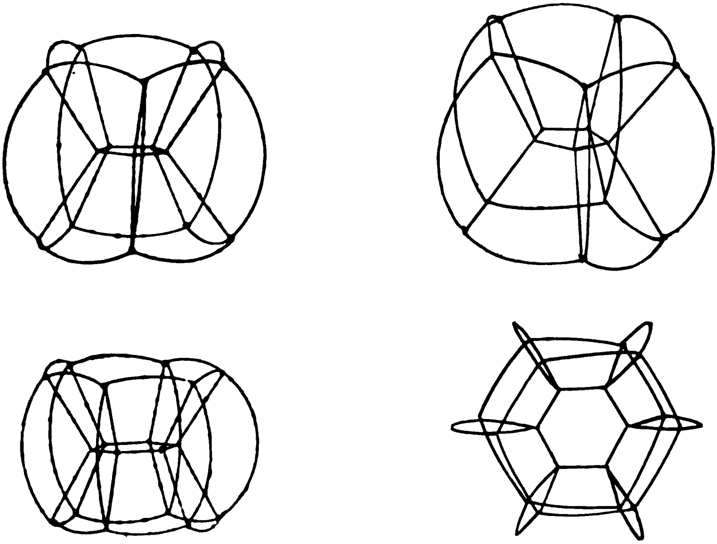


Fig. 22.

These "sections" are in reality photographs of the half groups when formed on a horizontal glass plate (4 inches square). A fountain-pen filler was used to make the equal half bubbles, by simply squeezing the indiarubber bulb quite flat, the nozzle having already been dipped in soap solution, and the glass plate well-moistened with solution. The single and double columns already described may be shown in the same simple manner (Fig. 24). By using the horizontal projection lantern these figures are plainly shown on the screen.

The forms taken by these clusters are governed by the conditions for equilibrium of the surface tensions at the intersections of the films. Not more than three films can meet in any one line, and if they are planes they will be equally inclined to one another at angles of 120°, while, if they are curved, then the law of equal inclinations holds between the tangent planes at all points of contact. Taking first the case of two bubbles which have coalesced into two segments united by a circular plane film, the tangent planes at any point of the line of contact of the curved surfaces will be inclined at 120° to each other and to the circular plane. It follows that the radius of the plane central film is $\frac{\sqrt{3}}{2}$ of the radius of the two equal spherical segments, the distance between their centres of curvature being equal to this radius, which is greater than the radius of the original separate bubbles in the ratio of $\sqrt[3]{4} : 1 \cdot 5$ or $1 \cdot 0 : 0 \cdot 9452$. Therefore the internal excess pressure (which is inversely as the radius of curvature) is reduced 4·5 per cent. by the coalescence. Measurements made on two bubbles gave under 5 per cent.

Three spherical segments in contact are each approximately five-sevenths of a whole sphere. The internal pressures therefore are now reduced to the proportion of $\sqrt[3]{7} : \sqrt[3]{5}$ or $1 : 0 \cdot 8933$. Now $0 \cdot 8933 = (0 \cdot 9452)^2$, so that the process appears to proceed by a geometrical progression, each linkage reducing the internal pressures in an equal proportion.

CONCENTRIC HALF BUBBLES.

An interesting application of the method of film formation by regulated drops was made in order to obtain a group of concentric hemispherical bubbles. It was desired to obtain them with a uniform difference of radii of 1 cm. between successive film surfaces. They were blown on the under side of the top glass plate of a smaller cubical vessel (30 cm. length of edge). A special form of nozzle was made with its rim flush with the surface on which the bubbles were to be blown. The part containing the narrow neck was of ebonite, and had a shoulder carefully fitted to the aperture in the glass plate. The dropping funnel and glass blowing tube were fitted tightly into the neck of the ebonite nozzle. Fig. 25 shows the arrangement with a group of ten half bubbles, the horizontal lines indicating the levels reached by black zones on the fourth day.

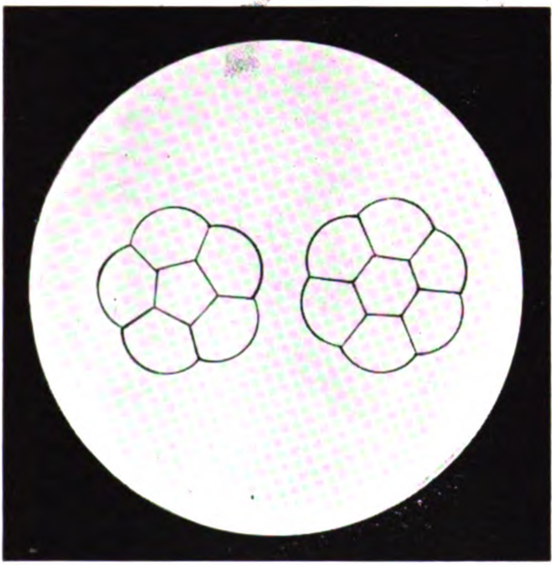


FIG. 23.

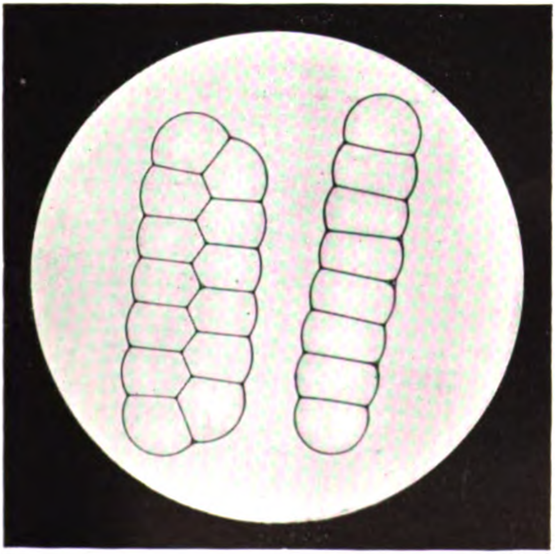


FIG. 24.

In order to determine the time at which each drop had to fall the volumes of the successive bubbles were calculated and their differences tabulated. Having fixed upon a suitable rate of blowing, the intervals between the drops that corresponded to the successive differences in volume were at once obtained. The rate of blowing was 216 c.c. per minute, so that the whole group took about 21 minutes to make, the intervals between the drops decreasing from about 4½ minutes at first to about half a minute at the end. The surfaces of the glass plate and nozzle were first moistened with soap solution.

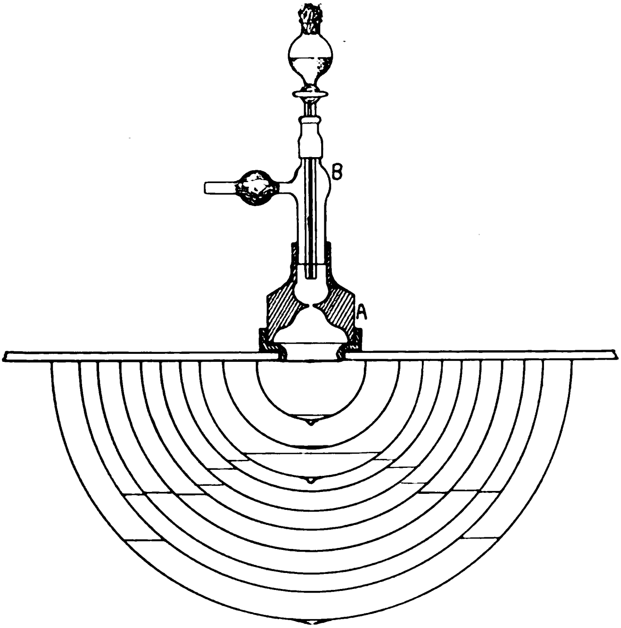


Fig. 25.

By the eighth day the black zones occupied 90 to 95 per cent. of the surfaces. Some experiments were made subsequently on the manner of diffusion of hydrogen through the group.

Film Contours Shown by Shadow.

When the shadows of these film complexes were thrown on a transparent screen every detail of their structure was clearly shown. For lecture purposes an arc lamp was used to produce these shadow-graphs, but in the laboratory a beam of parallel rays throwing the shadows on a sheet of tissue paper fixed on the side of the cubical glass chamber (containing the film-cluster), opposite to the source of light, gave means of making accurate measurements both of lines and inclinations of planes, as well as careful tracings for further study. Fig. 16 shows the shadow picture given by the quadruple column shown in Figs. 14 and 15.

WIRE MODELS OF BUBBLE CLUSTERS AND PLATEAU FRAMES.

The wire frames already mentioned can be used to build up the corresponding clusters. When they are dipped in soap solution linked films are formed on every plane outlined by the wires, because these are in the same stable relation that exists in the associated bubble group. It is then possible to insert bubble segments into the spaces between the planes and thus build up the actual cluster on a wire frame.

These form an interesting comparison with the well-known Plateau film groups, which were formed on wire frames made in the shape of geometrical solids—cube, tetrahedron, triangular prisms, etc. When such frames were dipped in soap solution linked film planes were obtained in beautiful regular formation, though not necessarily lying in the planes of the solid figure represented in skeleton by the wire, because these planes are not the stable planes of liquid films in contact. The film figures finally obtained were enclosed within the space of the skeleton figure. Thus the films initially obtained on the six faces of a skeleton cube are pulled inwards until they meet at an angle of 120° instead of the original right-angle. As a result a small vertical or horizontal square is formed at the centre with a pair of equally inclined truncated triangles radiating from each of its edges to the parallel edges of the cube; this gives eight equal truncated triangles linked by the central square. If a small bubble is included, this is drawn into a cuboid at the centre with the remaining planes as before. So with a tetrahedron, six planes are seen to be drawn inward one from each edge, to the four lines joining the apices to the centroid. This point is therefore common to the six planes which thus divide the space inside the tetrahedron into four equal and similar parts, like the conventional model of the four affinities of a carbon atom. A double tetrahedron has this appearance repeated at each end. Using an octahedron frame, we obtain six kite-shaped figures, in three perpendicular pairs, linking the three pairs of opposite vertices, and having their obtuse ends fitting alternately at the centre, while the acute ends stretch out to the apices. These six kites are connected to the twelve edges of the frame by twelve triangular planes, making eighteen films altogether.

A hexagonal prism is an interesting case illustrating the impossibility of getting more than three films to meet at one point. A wire prism of indifferent dimensions—say, with its hexagonal edge of the same order of length as its height—will give six triangular vertical films directed from each vertical edge towards the centre, where their apices are linked by a horizontal hexagon (Fig. 26). There are in addition six pairs of truncated triangular films linking top and bottom opposite hexagonal prism edges to the parallel central film hexagon, Fig. 26 (a, b, c). If the prism height is increased and this configuration preserved, then the central hexagon will decrease in dimensions and should vanish when the height of the prism is three time its hexagonal side, all the eighteen films then meeting at the centre. What happens during the process is shown in Fig. 26 (d): the central hexagon is preserved, the vertical planes are no longer triangles but have sides concave towards the apex, while the twelve remaining surfaces springing from the top and bottom edges are curved and look like a sort of hexagonal hour-glass—seen in profile (d). The condition governing all these arrangements is the equal inclinations of the tangent planes at the lines of intersection.

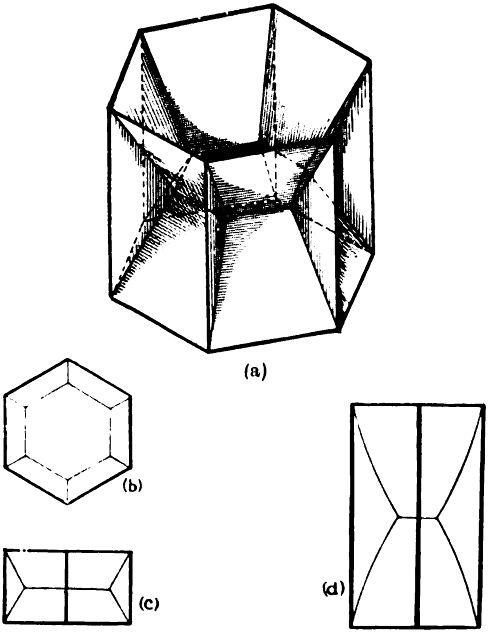


Fig. 26.

One interesting variation of this study was made by employing a frame composed of two thin platinum wire squares, linked by cellulose fibres vertically between the corners to form a cube, and supported by a pyramidal frame formed by four equal fibres from the upper cube corners joined together at the supporting apex. A similar pyramidal frame inverted was also formed below, the four fibres in this case being attached to a small lead weight. The resulting formation when the framework was dipped in soap solution is represented geometrically in the following three views, two elevations (*a*) and (*b*) and a plan (*c*) (Fig. 27).

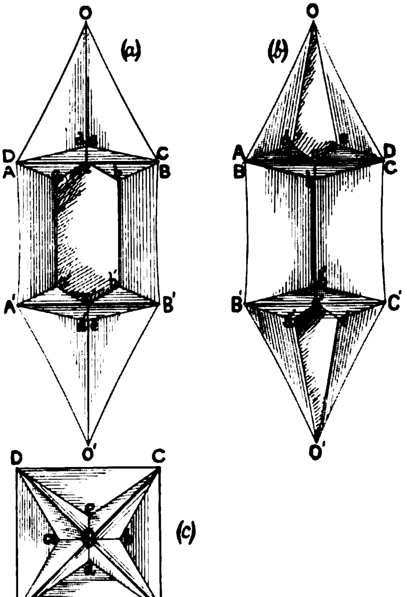


Fig. 27.

This is really a combination of the cube and the octahedron patterns (already described) obtained by equally dividing the octahedron horizontally, and adding the separated portions to the top and bottom of the cube. The central square plane of the simple cube is thereby pulled out to an irregular hexagon $a\,c\,b\,b'\,c'\,a'$ by the kite-shaped planes $\mathrm{O}\,d\,c\,e$, $\mathrm{O}'\,d'\,c'\,e'$, of the divided octahedron, with a consequent rearrangement of all the films at equal inclinations.

The truncated triangular films joining the vertical edges of the cube to the central (hexagon) plane then become very short. In Fig. 27 (*a*) they are shown by A *a a'* A' and B *b b'* B', while in Fig 27 (*b*) the other two are shown by C *b b'* C', and by D *b b'* D' behind. All four are shown in profile in Fig. 27 (*c*) by the lines A *a*, D *a*, B *b* and C *b*. The kite-shaped planes from the apices O and O' of the divided octahedron are Fig. 27 (*b*) O *d c e* and O' *d' c' e'*, which in Fig. 27 (*a*) are only seen in profile—i.e. perpendicular to the central irregular hexagon *a c b b' c' a'*.

The edges of the two pyramids (half-octahedrons) being O A, O B, O C, O D, O' A', O' B', O' C', O' D', then the triangular films drawn in from four upper edges are in Fig. 27 (*a*) and (*b*) O A *d*, O B *d*, O C *e* and O D *e* behind O C *e*. Similarly for the four lower edges. These triangular films also seen foreshortened in Fig. 27 (*c*) are indicated by the same letters.

From the upper horizontal edges of the cube there also spring four triangular films in two pairs, matched on opposite sides; those from the side edges A D, B C, Fig. 27 (*c*), go down to the central hexagon at *a* and *b*, Fig. 27 (*a*), while those from the front and back edges A B, D C, join up to the kite-shaped plane at *d* and *e*, Fig. 27 (*b*). The four films are thus shown in Fig. 27 (*c*) by A D *a*, B C *b*, and A B *d*, D C *e*. The same is repeated from the four lower edges of the cube, A' B', B' C', etc. There then remain two pairs of quadrilateral films linking the kites and the hexagon to the triangular films from the edges. These last are represented by A *d c a*, B *d c b*, in Fig. 27 (*a*), and C *e c b* in Fig. 27 (*c*); the other would be D *e c d* behind C *e c b*. The same is repeated on the lower edges.

The whole complex thus numbers 31 films: 12 from the cube edges, 8 from the pyramid edges, and 8 quadrilateral films, together with the kite-shaped pair and the irregular hexagon forming the backbone of the figure.

EXTENSIBILITY OF COLUMNS.

When groups of bubbles are first formed the films of which they are constituted are rather thick, because as a rule the segments are not expanded to any great size. Owing, however, to the free drainage down the intersecting channels, the excess liquid rapidly accumulates in the lower segments, and the weight thus localised distorts or breaks the group.

Some measures were therefore made of the elongations produced by added weights in various forms. In a single column it can be seen that the upper segments are longer than those lower down; with additional loading the extension increases until parting takes place. This is illustrated in Fig. 28 (which is drawn to scale). A column of 13 bubbles is shown at (*a*) when freshly blown with air containing 7 per cent. of hydrogen. After the hydrogen had diffused out for five minutes, the loss of buoyancy produced the sag shown in the second drawing (*a*), taken when rupture was imminent between the first and second segments; the measurements could not, therefore, be completed. The ratio of length to breadth of the longest segment (on the point of instability) is 1·17 to 1. Comparative measures are also shown in the two drawings (*b*) of the considerable contraction that takes place in an air-blown column between its early condition when uniformly coloured, with all superfluous liquid removed, and its final condition of complete blackness, and therefore minimum mass. The successive heights of the segments in a column half an hour after blowing (uniformly coloured, no excess liquid) were, counting from the top, 5·1 to 5·2 cm. for the first four; then in succession, 5·0, 4·8, 4·3, 4·2, 4·0, 3·8, 3·2—at which point contact was made with a larger segment on a fixed support.

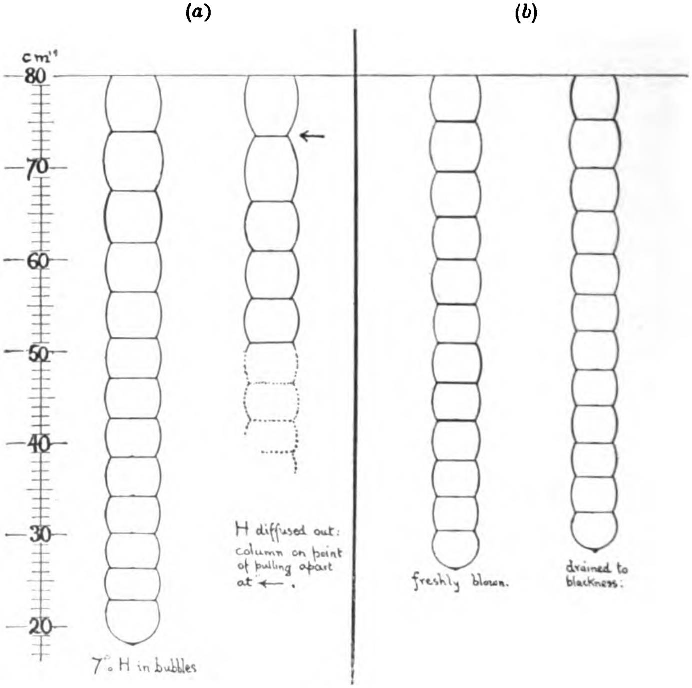

(a)
(b)
cm's
80
70
60
50
40
30
20
7% H in bubbles
H diffused out: column on point of pulling apart at ←.
←
freshly blown.
drained to blackness.

Fig. 28.

The long glass cylinder in Fig. 6 was chiefly used for these measurements. The sliding support rod was adapted for manipulating a separate light glass ring, on which small weighed pieces of steel wire could be hung. A graduated pull could be exerted by a small solenoid below. Tiny horizontal magnets were attached to the columns, and by their tendency to set in the meridian were able to retard irregular oscillations. For this purpose they were fixed to light cotton rings by being soaked in nitro-cotton solution and dried.

A column of six segments each of 150 c.c. was elongated by one-third of its length with a weight of 0·136 gram; the original length, free of superfluous liquid, being 31·0 cm., and the maximum elongation, 11 cm.; the smallest additional weight then broke the column. The same weight, hung on a column of five bubbles 23 cm. long, caused an elongation of 8·3 cm., or 35 per cent. On the removal of a drop of liquid weighing six centigrams a contraction of 2·3 cm. followed.

A column of three segments gave the following result: with a weight of ·075 gm. its initial length was 14·7 cm., and the elongation produced was 1·35 cm.

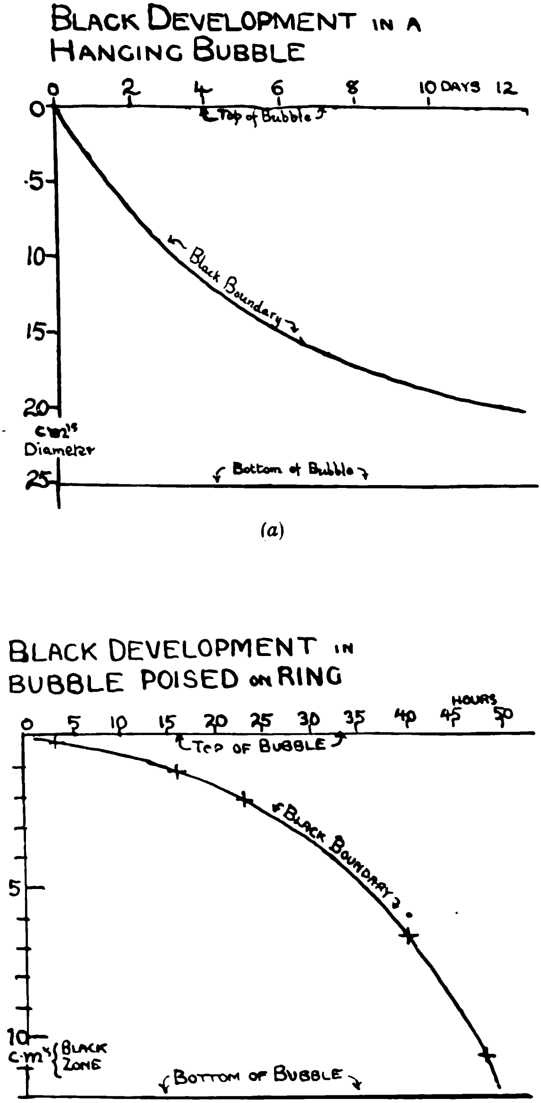
The following values were obtained from the gradual increase of load when a loss of buoyancy occurred by diffusion of hydrogen from a column of eight equal segments of 150 c.c. blown with 7 per cent. hydrogen. The initial length of 35 cm. was increased to 39·5 cm. when a weight of 7·5 centigrams was hung on. When the hydrogen had diffused out the total length became 40·3 cm., or an extension of 0·8 cm. from an additional weight of 8·4 milligrams. These results are recorded in the following table (I.).

TABLE I.

n	l	w	e	wl	x	wl/x	E
	cm.	gm.	cm.				
6	31	·136	11	4·22	·355	11·9	·383
5	23	·136	8·3	3·13	·361	8·7	·377
,,	23	·136–·006	8·3–2·3	2·99	·261	11·5	·498
3	14·7	·075	1·35	1·10	·092	12·0	·817
8	35	·075	4·5	2·63	·129	20·4	·583
,,	35	·0834	5 3	2·92	·152	19·3	·552

Where n is the number of segments in the column; l is its length in cms.; w is the attached weight in gms.; e is the elongation in cms.; x is the elongation per unit length of column; and E is the corresponding value of Young's modulus.

It will be noticed, on comparing the fourth set of these observations with the first set, that (roughly) halving the weight and length reduced the elongation per unit length to one-fourth, as might be expected. For, $x = \frac{e}{l}$ and $W = E\frac{e}{l}$, hence for the first case


BLACK DEVELOPMENT IN A HANGING BUBBLE
0
2
4
6
8
10 DAYS
12
0
·5
10
15
20
cm's Diameter
25
Top of Bubble
Black Boundary
Bottom of Bubble
BLACK DEVELOPMENT IN BUBBLE POISED ON RING
HOURS
0
5
10
15
20
25
30
35
40
45
50
5
10
c·m's
BLACK ZONE
TOP OF BUBBLE
BLACK BOUNDARY
BOTTOM OF BUBBLE

(a)

(b)

FIG. 29.

$W l = E e$; for the second case, $W' l' = E' e'$, or $W l = 4 E' e'$. $\therefore 4 E' e' = E e$. Now in the Table I. $E' = \cdot 817$, $E = \cdot 383$, or approximately $E' = 2 E$, hence the last equation becomes $8 e' = e$, or in terms of x, $8 x' \frac{1}{2} l = x l$, whence $4 x' = x$.

GAS TRANSFERENCE THROUGH FILM COMPLEXES.

Columns and clusters of bubbles generally reach the "black" stage with great rapidity by the action of the channels (Gibbs rings) of liquid where the segments join together. The black development of an ordinary hanging bubble is different from that of a similar bubble resting on a ring, because in the latter case the action of the Gibbs ring is to assist gravity in withdrawing liquid from the film and thus to accelerate the thinning process, while in the former case the action of the Gibbs ring is against gravity, and thus retards the withdrawal of liquid from the film. The two curves in Fig. 29 show this difference very clearly: the black boundary on the poised bubble descending at an ever-increasing rate, as it approaches the Gibbs canal present on the support ring; while in the hanging bubble the rate of fall continually decreases. (This is more noticeable when liquid is allowed to accumulate below while the thin black area extends downwards. In the absence of suitable drainage, the coloured zone then becomes continually thicker.) When however a bubble was provided with two Gibbs rings, one above and one below, the rate of fall of the black boundary was almost linear for the greater part of the time.

This was arranged by attaching a small bubble under a large one, supported as usual from a nozzle above. The small bubble was kept steady by a smaller glass ring underneath. The nozzle was 8·3 cm. in diameter, and the size of the small attached bubble was adjusted so that the diameter of the Gibbs ring of contact between the two bubbles was about the same. The greater bubble was 40 cm. high between the equal Gibbs rings top and bottom, viz. where contact was made with the nozzle above, and the small bubble film below. The graph of "black fall" plotted with time was very nearly straight for 2½ days, by which time the black area had extended over three-fourths of the surface. After this it spread more slowly, and took another day and half to reach the lowest point. The form was therefore intermediate between curves (*a*) and (*b*) of Fig. 29.

Now, in the more complex clusters there are many channels all connected at various inclinations, therefore not only is any excess liquid quickly discharged, but the films themselves are quickly drained to the "black" state.

This is the most favourable condition for the study of gas transference, as the film is then at its minimum thickness. The resulting contraction has been measured in the case of straight columns and other complexes by photographs taken periodically. These afford

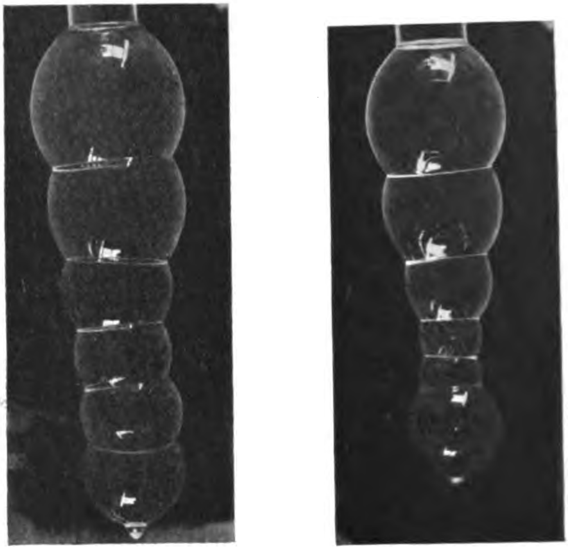


FIG. 30.

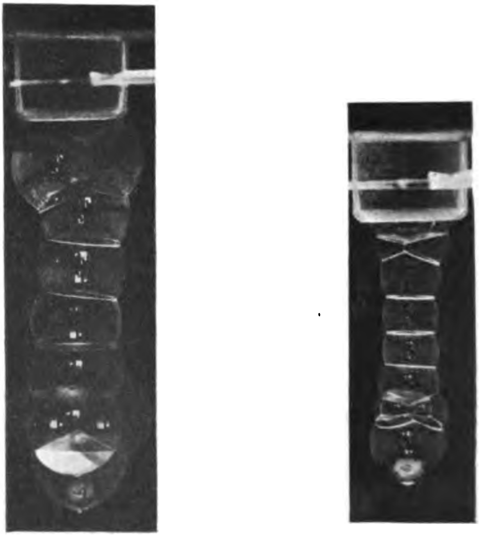


FIG. 31.

direct evidence of greater rate of transference in the smaller bubbles; for when records are taken of a column of unequal segments it is easily seen that the initial inequalities are accentuated as time goes on. Figs. 30 and 31 are reproductions from photographs of two small columns at intervals of nine and seven days respectively; the volumes of the segments varied from 30 to 100 cm.

Large black volumes were more difficult to deal with on account of irregular oscillations; their relatively small mass when black was insufficient to stabilise them against any local convection. For example, in the 200 litre globe already described a column was blown consisting of six segments, the upper five of which had each a capacity of 1½ litres, while that of the lowest was 3 litres. In one day it became wholly black with the exception of a thick coloured area that extended up over 30 per cent. of the lowest bubble, the weight of which was sufficient to keep the whole hanging vertically, although some of the junction planes were slightly inclined. By the sixth day the colour had diminished to a zone 5 cm. wide. This reduced load could not keep the column straight, and some of the oscillations that occurred displaced the drop on the lowest bubble as much as 15 cm. from the vertical, while a vertical contraction of 15 mm. had accrued, followed in the next two days by a further contraction of 6½ mm. The coloured zone was now a disc of 2 cm. diameter, and the column had become so curved that an unusually large oscillation caused the lowest bubble to touch the globe, with the result that only the two uppermost segments remained, the junction plane between them being inclined at 30° to the horizontal.

A light glass ring was therefore arranged below to keep the column stationary. It was sealed to a 3 mm. glass rod bent round in a bow, roughly to follow the contour of the globe, and then secured to a vertical glass rod that could slide air-tight in the rubber stopper at the neck. The contraction of the column due to the gas transference, however, went beyond the limits through which the glass rod could be raised. The segments therefore became thinner and more strained, until on the 89th day separation took place between the third and fourth. The glass ring thereupon relaxed a distance of 8 mm., the amount by which the glass bow had been strained by the pull of the black column.

In some cases the various segments were measured daily by a cathetometer and horizontal sliding telescope. If D is the diameter of a bubble segment at the equator, and d the diameter of the plane circular ends, while h is the height, then the volume is given by

$$V = 1\cdot0472h\,(0\cdot4\,D^2 + 0\cdot2\,Dd + 0\cdot15d^2).$$

When the segment is unstrained, $d = D - 0\cdot268h$, so that

$$V = 1\cdot0472h\,(0\cdot75\,D^2 - 0\cdot130\,Dh + 0\cdot01h^2);$$

only D and h need therefore be measured.

These relations were found to agree very well with the dimensions of segments of known volume.

The area of the curved surface through which the gas transference takes place (there being practically no loss through the plane ends) is given for a bubble whose centre of curvature is not in the axis of revolution by

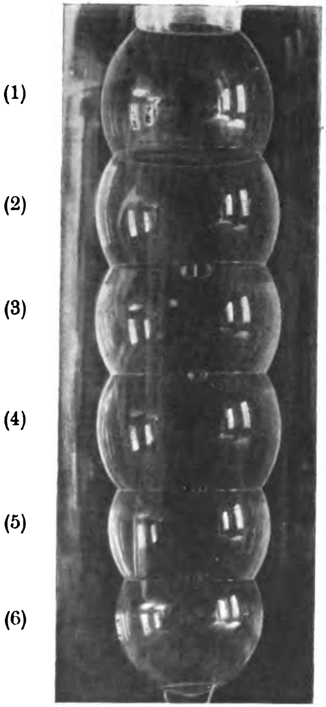


Fig. 32.

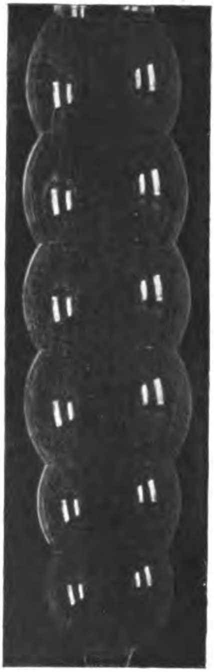


Fig. 33.

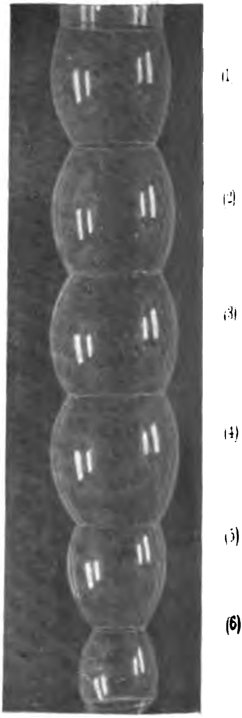


Fig. 34.

$$S = \frac{\pi^2}{3} Dh - \left(2\frac{\pi^2}{3} - 2\pi\right) h^2 = 3\cdot289\ Dh - 0\cdot295\ h^2.$$

It must be noted that an uncontracted bubble segment has been assumed. This, however, was no longer the case when it became necessary to fix the column at both ends (to a nozzle above and a glass ring below). As the contraction proceeded therefore the total length did not alter, so that h remained practically the same throughout; but as the segments shrank D and *d* continually diminished. Three illustrations of this are shown in Figs. 32–34. They are taken from a column of six bubbles in the 200 litre globe already described. The four top bubbles were of equal volume, while the two lowest were half the volume of the others. The third segment being between two similar ones remained symmetrical to the end, and thus gave more reliable results. The contraction finally resulted in the column pulling apart between the fifth and sixth bubbles on the 64th day. The upper five were thus left free, but the oscillations were so persistent that only a blurred photograph was possible; this was, however, sufficient to give some measures of the free segments for comparison with those obtained when strained. Fig. 32 was taken at the start, Fig. 33 on the 16th day, and Fig. 34 on the 64th day before partition took place.

The volumes and surfaces for the third segment selected for calculation were as follows (Table II.):—

Black Six-Bubble Chain.

TABLE II.

DIMENSIONS OF SEGMENT (3).

Time	1 (Fig. 32)	16 (Fig. 33)	64 (Fig. 34)	Days
Volume	1133	1029	543	c.c.'s
Curved Surface ...	360	350·5	258	(cm.)²

TABLE III.

AIR TRANSFER THROUGH SEGMENT (3).

Volume		1000	750	650	600	c.c.'s
Curved Surface ...		346	305	288	279	(cm.)²
Rate of Transfer	c.c.'s per day	7·6	11·3	14	16	through whole surface
		0·022	0·037	0·049	0·059	per (cm.)²
Volume Contraction ...		0·76	1·51	2·15	2·67	per cent. per day

When the values were plotted with time, contraction curves were obtained for both volume and surface. By taking tangents to the volume and time curve at successive intervals of time, the corresponding rates of gas transference $\frac{dV}{S}$ were deduced.

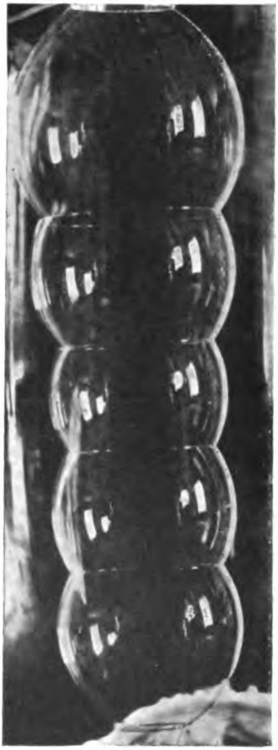


Fig. 35.

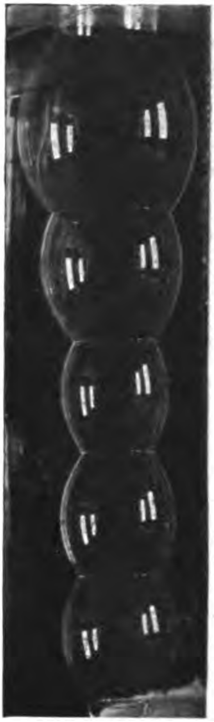


Fig. 36.

The third segment thus gave the values shown in Table III. for air transference through the black film composed of $3\frac{1}{3}$ per cent ammonium oleate and 33 per cent glycerin.

Three sets of these columns in the 200 litre globe were tried. The first was the largest in volume, and took 101 days to pull apart. Figs. 35 and 36 show the appearances at start and finish, the total length meantime had been reduced from 53·3 to 50·9 cm.; after this the free bubbles remained on the nozzle a further 53 days. They were then replaced by the second set already described, which took 64 days before it divided. The third set which followed was more uniform, consisting of one segment, top and bottom, each of whose volumes was half a litre, while the four in the middle contained one litre each. This set parted in 40 days.

Very good black columns were also obtained in the long glass cylinder (Fig. 6). With this it was possible to prevent undue straining of the segments by carefully raising the support ring when necessary. The cylinder was long enough to get in ten segments, each of over 250 c.c. A thin thread of stranded cellulose (artificial silk) was sometimes fixed both at the nozzle and support ring to lie along the outer contour of the column from top to bottom. This not only stabilised the column while being blown, but accelerated the drainage and black development, because any excess liquid in the Gibbs ring channels of the junction planes was quickly drawn into the capillary canals of the silk. The result was that the black stage was complete in one day, and there was entire absence of any deformation from superfluous liquid remaining in the junction rings. One set took 47 days before the shrinkage caused the segments to part, the support ring not having been raised. The ratio of length to breadth of the equal segments two days before this occurred was 1·5 : 1, as compared to 1·17 : 1 in a similar coloured column of like dimensions, but without any cellulose thread, and dragged apart by loading (see Fig. 28).

The segment volumes of another set diminished during the first fortnight from about 270 c.c. to 225 c.c., and the curved surfaces from 145 to 120 square centimetres. The calculated rates of air transference obtained from the contraction curves, are given in Table IV.; the solution used contained 5 per cent ammonium oleate and 50 per cent glycerin.

TABLE IV.

Volume		265	250	235	c.c.'s
Curved Surface		143	139·5	136	(cm.)^2
Rate of Transfer	c.c.'s per day	1·83	3·62	5·24	through whole surface
		0·013	0·026	0·039	per (cm.)^2
Volume Contraction ...		0·69	1·45	2·23	per cent per day

Much higher values were given by a black column of five equal segments, initially 161 c.c., very completely drained by two cellulose strands on opposite sides of the bubble contours. No glass ring support was employed, so that the column contracted freely without the segments being strained out of their normal curvature. In four days the volume diminished 30 per cent. The mean rate of air transference being nearly 11 c.c. per day when each segment had a volume of approximately 125 c.c. The corresponding value of the curved surface was about 80 (cm.)^2, so that the loss through unit area was 0·14 c.c. per day. The solution used for this contained 1 per cent ammonium oleate to 10 per cent. of glycerin.

GAS DIFFUSION THROUGH BLACK FILMS.

The rate at which gases diffuse through liquid films was found to be very much greater than the slow escape caused by the small excess-pressure by which bubbles are distended. The latter only becomes appreciable some days after the black stage is reached; but if a thick coloured air bubble has a small percentage of hydrogen blown into it, a contraction takes place in a few minutes; or, conversely, an expansion is quickly produced by circulating some hydrogen in the closed vessel in which the bubble is hanging. In the same way when a plane film was formed across a cylindrical vessel containing only air and some soap solution, and hydrogen was then circulated in the space between the film and the neck, a steady movement of the film towards the neck at once began; the hydrogen passed through to the enclosed space beyond the film more quickly than the contained air diffused out. The reverse process was easily carried out by first filling the bottle with pure hydrogen, and expanding the film as before. A current of clean air was then circulated on the same side of the film as before; the resulting movement of the film was now away from the neck, because the volume enclosed beyond the film lost hydrogen more quickly than air could diffuse in. When oxygen was used instead of air the motion was more rapid.

When these experiments were made after the black stage was reached, the diffusion was sufficiently rapid to cause a large distortion of the plane film, due to partial adhesion round the periphery of the film, although the glass walls had previously been well moistened. For the laboratory measures of the relative diffusion rates of various gases, an 8 litre bottle, 19 cm. in diameter, was calibrated, and the movements of the film (which would thus have an area of some 300 cm.^{2}) were noted for successive small time intervals. The stopper was fitted with inlet and outlet tubes, of which the former was long enough to reach the bottom, and could also slide easily in an airtight fitting. The film was thereby started by pushing the inlet tube down into a small quantity of soap solution in the bottle, and then withdrawing it gradually as the gas current expanded it to the desired position. The tube was then pulled just through the film, the current nearly shut off, and the whole left to settle until the black zone had developed sufficiently in the film. The gas to be circulated was then connected and turned on, and the movements of the film noted. The end of the outlet tube was kept within a cm. of the film surface to maintain a thorough circulation.

With hydrogen in the vessel, and when the film was half black and half silvery (next in thickness to black), and air was circulating at 130 c.c. per minute, the volume enclosed by the film decreased at a fairly uniform rate of 1 litre in 20 minutes. After this the movement became slower as the percentage of hydrogen diminished in the space beyond the film. Finally, 5·70 litres of hydrogen originally present were found to be replaced by 1·40 litre of air. In another experiment, with the film almost completely black, 6·23 litres of hydrogen diffused out and were replaced by 1·35 litre of air.

The maximum rate of diffusion could not be realised in this wide vessel because of the film distortion that was produced when the rate of circulation was increased. To overcome this a vessel only 8·5 cm. in diameter was used (Fig. 37), in which the ordinary stopcock was replaced by a two-way cock A, while an inlet and outlet fitting was fixed in the rubber stopper. The outlet tube as before could slide easily in its airtight neck. The vessel was filled with hydrogen coming in through A and leaving at B. The film was obtained (after first moistening the walls of the vessel) by tilting a drop of the soap solution into the neck at A. The gas entering there

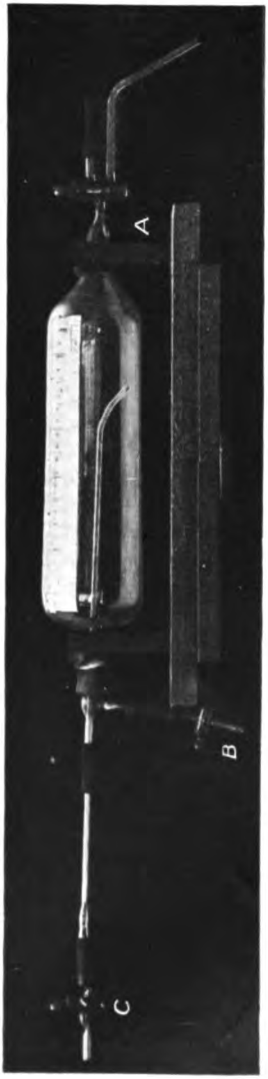


Fig. 37.

at once formed a film and pushed it along to the desired position. A was then closed and the film left to become black. Meanwhile a small current of hydrogen was continued through B and C to prevent any subsequent disturbance of the film, such as by an alteration of temperature.

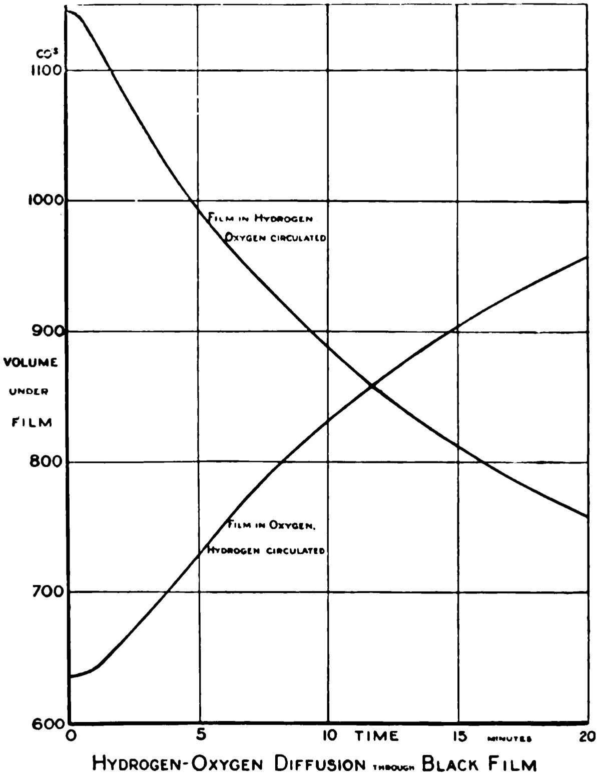


Fig. 38.

Before starting the air or oxygen current through B and C the film was lubricated by a slow complete revolution of the vessel, whereby the small amount of liquid inside flowed all round the walls. This caused some diminution of the black area, which was measured and allowed for. During the subsequent diffusion the sliding tube was adjusted so that its open end was kept within a centimetre of the moving film. Small measured samples of the mixed gases behind the films were withdrawn at intervals for analysis through the two-way cock A.

The maximum rate of diffusion of hydrogen above that of air was found to be 42 c.c. per hour per cm^2. When oxygen was used instead of air the value obtained was 50 c.c. of hydrogen per hour per $cm.^2$ greater than the oxygen going the other way; and 1145 c.c. of hydrogen passed out, while 632 c.c. of oxygen went in. When the reverse process was tried 1173 c.c. of hydrogen went in through the black film, while 635 c.c. of oxygen diffused out. The two operations involved the passage of the film 7 cm. along the cylinder in the first case, and the same back again for the reversal. Fig 38 gives the graphs of the movement of the film, and shows the variation of the enclosed volume, with the time. The rate of relative diffusion at any time is given by the slope of the tangent to volume-time curves. This was found to decrease as the proportion of oxygen under the film increased.

The contraction of bubbles in air when blown up with from 10 per cent. to 30 per cent. of hydrogen was measured in order to deduce the diffusion rates through coloured films. A much lower value was obtained; thus a bubble coloured steely blue to pale amber and containing 16 per cent. of hydrogen showed a rate of contraction of approximately $\frac{2}{3}$ c.c. per hour per cm^2. Measures were also made with half bubbles blown on the roof of a glass chamber in which a proportion of hydrogen was circulated; and also by comparing the contractions of similar bubbles when hanging from a nozzle and resting on a ring. This last was done to correct for the distortion of figure produced by the buoyancy of the contained hydrogen.

HYDROGEN BUBBLES BLOWN UNDER SEVERAL ATMOSPHERES.

Bubbles that have thinned to blackness contract at an ever-increasing rate by the continual transference of the enclosed gas through the envelope. It was mentioned in last year's Discourse that this contraction is greatly retarded when the bubbles are formed in an enclosure in which the pressure has been raised, the excess-pressure distending the bubble becoming then of relatively smaller proportion to the total pressure. Further experiments have shown that a black bubble in hydrogen under 6·3 atmospheres pressure took 100 days for its diameter to contract from 7·7 to 4·0 cm., whereas only 7 to 10 days is needed for the same contraction to take place in a similar bubble at atmospheric pressure. When air was used instead of hydrogen the difference was equally marked; thus an 8 cm. bubble in a vessel charged to 7 atmospheres took 4 months to diminish 1 cm. in diameter, the same contraction at atmospheric pressure taking only about 14 days.

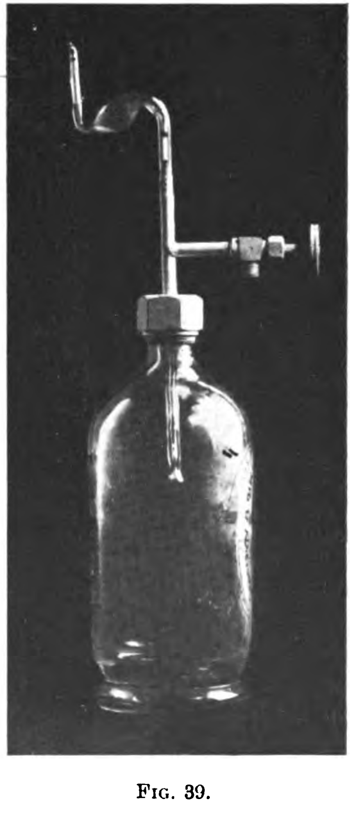


FIG. 39.

Suitable vessels for these measurements are afforded by the strong glass bottles used to hold sulphurous acid. A screwed brass collar was cemented to the neck, and to this a brass T-piece was secured by a gas-tight union. The glass nozzle for supporting the bubble was cemented into the vertical arm of the T-tube, and sealed to a thick glass bulb above bent over horizontally to hold some soap solution. The gas used was admitted by the valved horizontal arm of the T-piece, a hole being made at this level into the inner glass tube to give access through the nozzle, which had a diameter of about 1 cm. (Fig. 39). Just within its lower end a constriction was formed. The apparatus was tested and found tight under 10 atmospheres pressure.

The pressure was first adjusted to about 6 atmospheres, and a drop of soap solution was decanted into the constriction; on admitting more gas a bubble was expanded on the nozzle and the pressure thereby raised to 6½ atmospheres. The black stage then developed very much as usual, and the gas transference was determined by periodic measurements of the contraction.

Table V. gives the rate of transference obtained in hydrogen at 6½ atmospheres compared with the values at 1 atmosphere. The numbers given are calculated for diameters of 4, 6 and 8 cm. The second column gives the internal excess pressure P of the soap bubble (in mm. of water) measured at atmospheric pressure. The proportion F of this to the total pressure in the bubble vessel is shown in the third column (in units of 10^{-6}) both for 1 atmosphere and 6½ atmospheres. The observed rates of gas transference R are similarly shown in the fourth column.

TABLE V.

Diameter cm.'s	P mm.'s H_2O	$F \times 10^{-6}$		R c.c.'s	
		(*a*) 1 atm.	(*b*) 6·5 atm.	(*a*) 1 atm.	(*b*) 6·5 atm.
4	0·330	32·16	4·80	0·22	0·0825
6	0·220	21·44	3·20	0·14	0·041
8	0·166	16·08	2·40	0·10	—

Thus it may be said that the longevity is increased in the proportion of the numbers given in the last two columns, in this case, approximately as the square roots of the relative pressures under which the bubbles were blown.

For experimental assistance I have to thank W. J. Green, B.Sc., and for reading the proofs and checking the calculations, I am indebted to J. D. H. Dickson, M.A., Senior Fellow of Peterhouse College, Cambridge.

[J. D.]

LONDON: PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
GREAT WINDMILL STREET, W.1, AND DUKE STREET, STAMFORD STREET, S.E. 1

WEEKLY EVENING MEETING,

Friday, January 24, 1919.

THE HON. RICHARD CLERE PARSONS, M.A. M.Inst.C.E.,
Vice-President, in the Chair.

COLONEL ANDREW BALFOUR, C.B. C.M.G. M.D. R.A.M.C.

One Side of War.

COLONEL BALFOUR commenced his discourse with the following remarks:—

"It is my purpose this afternoon to give you a demonstration on what I have called 'One Side of War.' It is the medical side concerning which I will speak, that being the only aspect of which I have any special knowledge, and, even so, such knowledge is confined to certain sections.

"Now the Medical Side of War, like all Gaul, is divided into three parts: the strictly medical, embracing hospital administration, clinical and ambulance work of all kinds; the sanitary, or, as it should be called, the preventive part; and last, but by no means least, the scientific, which is also largely of a preventive nature.

"In addition to France and Flanders, of which I saw very little, I have been privileged to visit all the important tropical and sub-tropical theatres of war. As a member of the Medical Advisory Committee I went in 1915 to Egypt, Mudros and Gallipoli. In 1916 I was with that Committee once more in Eygpt and also in Macedonia, Malta, India, Mesopotamia and Persia. Leaving Mesopotamia early in 1917 I returned to India, thence to England, and thereafter accompanied Major-General Pike to South and East Africa, my itinerary in the latter comprising Portuguese Territory, the late German Colony, British East Africa and Uganda. From Uganda I travelled viâ the Sudan to Egypt, and finally, at General Allenby's kind invitation, proceeded to Palestine to see the anti-mosquito operations there.

"As it is my intention to show you something of all three divisions of the Medical Side of War in most of the countries I have mentioned, it is obvious that the sooner I turn from these notes to the screen of the type of the average British soldier—a type I can best explain by a short, but what is even better, a true story.

"A young lad lay seriously ill in one of the great hospitals of Malta. The consulting physician approached his bed and said to him, 'Well, my boy, how are you to-day?'

"The patient replied as he had always replied when asked this question throughout many weary days. 'In the pink,' he answered feebly, and, having so answered, he lay back quietly and died."

[A. B.]

WEEKLY EVENING MEETING,

Friday, January 31, 1919.

THE HON. RICHARD CLERE PARSONS, M.A. M.Inst.C.E.,
Vice-President, in the Chair.

PROFESSOR H. H. TURNER, D.Sc. D.C.L. F.R.S. F.R.A.S.

Giant Suns.

WE have all been fascinated by Giants, from the times we read of Jack the Giant Killer in our childhood to those more recent when we read of the exploits of Lieut. Warneford and his successors in their fights with the Giant Zeppelins.

I make no apology for shortening my title a little from what astronomers might expect. I might have chosen "Giant and Dwarf *Stars*," but stars are suns, as we shall see presently, and though I shall include "dwarf" suns, the real dwarfs of science are the tiny atoms at the other end of the scale of investigation—or rather, the electrons into which they have been broken up.

How shall we gauge the size of a star to see whether it is a giant? We must know two things: first of all, the apparent size of its disc, and secondly, its distance. In the old days it was thought that the size of the sun could be estimated from one of these only—from the size of the disc. Lucretius,* following Epicurus, believed that the sun was a small body. He arrived at this conclusion by neglecting entirely the consideration of the distance and judging by the appearance to our senses.

Now, without attempting to decide whether the sun is the size of a soup-plate, or of a threepenny-bit, or what is the size that it seems to be, we may remark that it seems to be about the same size as the moon, and that by Lucretius' principle the sun and moon ought to be of the same actual size.

However, we now know that the sun is 400 times bigger than the moon, because its distance is 400 times greater. We have measured the distance of the sun and found it to be nearly 100 million miles, and we have measured that of the moon and found it to be nearly a ¼ million; and since the discs appear of about the same angular size (viz. about $\frac{1}{100}$th part of the distance in each case), we know that the sun must be about a million miles in diameter and the moon only about 2500 miles, using round numbers throughout for simplicity.

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* "Nec nimio solis major rota nec minor ardor
Esse potest, nostris quam sensibus esse videtur."
Lucret., *De Nat. Rev.*, v. 564–5.

Thus the sun is a veritable giant compared with the moon, in spite of the similar apparent size of the discs; but this we discover only when we have measured the distances.

The sun and moon present to us large discs which we can study in detail, and the study of the disc of the sun by means of the spectroheliograph has had new triumphs which, owing to the War, have not yet been seen in this room, so that I may be pardoned for exhibiting one.

(Mount Wilson spectroheliogram shown.)

But when we come to the stars there is no disc. If one seems to see discs for these objects, the appearance comes through the imperfections of the telescope. Hence it would seem to be superfluous to inquire about their distance. When Mark Twain had been roughly handled at Niagara Falls, and the doctor reported that only sixteen of his wounds were mortal, he said "he did not care about the others." In the same way we might argue that since the stars have no appreciable discs we need not care about their distances.

That, however, was not the attitude of men of science. They went to work to measure their distances, and though the difficulties were heart-breaking they were attacked and overcome. Here is a table showing a sensible fraction of the life-work of an eminent Scotsman, Sir David Gill, observing at the Cape of Good Hope. It includes the famous α Centauri, the first star to have its distance measured at all, which again was by a Scotsman, Henderson, also observing at the Cape of Good Hope.

GILL'S PARALLAXES OF BRIGHT STARS.

Star	Parallax	Distance in Light-Years
	"	
Sirius	0·37	9
Canopus	0·00	?
Rigel	0·00	?
α Centauri	0·75	4
α Eridani	0·04	80
β Centauri	0·03	100
α Crucis	0·05	64
Spica	0·00	?
α Pisc. Austr.	0·13	25
β Crucis	0·00	?

Sir David Gill was accustomed to describe the difficulties of determining that distance by saying that it was like trying to measure the size of a threepenny-bit two miles off; and we remember his delight when his chairman on one occasion asked, "Who but a Scotsman would care about a threepenny-bit two miles away?"

But you will see that this star is the nearest and therefore easiest of all; while some even of these brightest stars gave no result even to a patient Scotsman. Those that are measurable show that they are so far off that light takes years to come to us from them—from some four years; from others hundreds of years; from those with no measurable result, thousands of years at least.

Thus we began to obtain a little knowledge of the distance of the stars. The method used for these measurements was the usual method of Parallax, which we may illustrate by two searchlights trained on the same Zeppelin. Knowing their distance apart and the angles at which they are sending their beams of light, those working the apparatus can draw the triangle to scale and thus tell the height of the Zeppelin.

Now replace the two searchlights by two telescopes—one on one side of the earth's orbit round the sun, and the other on the other side; they cannot be there simultaneously, but the star will wait six months for us to move round or even longer. The angle at the Zeppelin becomes however woefully small as we suppose it to mount to the stars. It is twice the angle which seems to separate earth and sun as seen from the Zeppelin, and it is this angle which is represented by the diameter of "a threepenny-bit two miles away." From the distance of the nearest star the sun and earth might appear as a close double star, of which there are many examples in the heavens, though our little earth would probably be too faint to be seen, even from the nearest star-spectator.

There would be no such difficulty in seeing the sun, but since his diameter is only $\frac{1}{100}$th part of the distance between earth and sun, which has itself shrunk to almost imperceptible dimensions, it is easy to realise that the disc of the sun would have disappeared completely, as does any disc of the stars to us, even with our largest telescopes.

Since we have imagined ourselves to mount far upwards to α Centauri, whence the sun and earth would represent a close double star, let us retain the conception a moment longer in order to note a useful fact. Watching long enough we should see the pair moving across the heavens, while at the same time the earth would revolve round its mighty companion. The sun would proceed therefore very much more steadily than the earth. The sun's path is nearly straight, while the earth takes a wavy path, or more correctly a corkscrew path. If the masses of the two bodies were more nearly equal, the two paths would be more nearly alike, and both wavy.

By observing such movements (for we can observe the movements of stars) we infer whether one component of a double star is more massive than the other or whether they are nearly equal; and it is found that there is never any very great disparity in mass between the components. Their masses are closely similar, like those of pebbles on a beach. But this tells us nothing about the sizes of the minute discs. Have I gone too far perhaps in saying there is no disc visible in any star? Nebulæ do show discs, and though they are nebulæ and not stars they may become stars. Here is a beautiful picture of the "Owl" nebula, taken with the Mount Wilson five-foot. It shows a beautiful circular disc with a star in the centre. Comparing Lord Rosse's drawing of it made with his wonderful six-foot reflector, we see the much greater perfection of the photograph; but this is chiefly due not to the inferiority of Lord Rosse's instrument, which was a marvel of engineering skill, but to the greater efficiency of photography compared with the eye. The new secrets wrested from the stars have chiefly come, not from the increase in size of telescopes, but from the new appliances attached to them, such as the photographic plate, the spectroscope, and by this time many others. The lines in the spectra of stars tell us what the stars are made of, how they may be classified accordingly, how fast they are moving, how bright they really are (this is an amazing recent discovery), and by inference how far away, and may yet have other surprises in store. For the moment we are chiefly concerned with the classification. The Harvard system gives us a number of classes denoted by the capital letters O B A F G K M R N. The fact that the order is not quite the same as that of the alphabet represents a revision of early ideas, chiefly due to the gradual accumulation of intermediate types, which make a nearly continuous series.

Now a series of stars in order is probably a representation of growth; just as the growth of trees may be illustrated by selecting various stages from the same wood, an illustration originally given by Sir W. Herschel. But we have seen a tree grow, and we know independently that it grows up from the acorn through the sapling to the giant oak; while we have not had time to see a star grow and were thus in ignorance whether the changes are from B towards M or from M towards B, though by this time we have an immense number classified. The classification has been largely the work of an American lady, Miss Cannon. I am told that there is a man who can deftly straighten rifle barrels—he gives a glance along the barrel, a tap with a hammer, and lo! it is straight. His value is recognised at some £15 a week. Miss Cannon has the same deftness with spectra—but I fear that (to judge from the report of the Board of Visitors of Harvard Observatory) her great skill is not so appropriately rewarded.

Now it is obviously important to find out, if we can, which is the direction of a star's growth, and we seemed to have an important clue when the spectral classification was connected with the temperature of a star, or rather its *surface* temperature, which is all we can get at. The outside is the coolest, just as the edges of a plate of porridge are the coolest, as most of us have learnt by early and rather painful experience. And yet the outside of a star is hot enough. The temperature is again estimated from the spectrum, though this time not from the lines but from the relative intensities of the ends, and the O B A end is undoubtedly hotter than the other. We may give as illustrations 15,000° for B, 5,000° for G, and 2,500° for M. Does this settle the matter? We know that there is a general tendency for all bodies to cool which points to the direction C B—M N as the order of events; but it was also known that under the stress of gravitation a star might rise in temperature, in which case the growth might be the other way. Still the former alternative commended itself more generally; and when Prof. W. W. Campbell found that the velocities of stars (also determined with the spectroscope) were smaller for type B than for type M, the facts were interpreted to mean that a star moved more quickly with advancing age (because M stars were older than B). The idea that the life of a star was spent in passage down the series O B—M was indeed pretty firmly established at the time when the revolution came.

The revolution began with the advent of a young American research student, Mr. H. N. Russell, to Cambridge in 1904–6. It is to the credit of Mr. A. R. Hinks that he made so much of this brilliant young student, setting him on the way to determine the distances of a number of stars by photography with the instruments which he (Mr. Hinks) has spent much time and labour in perfecting. This was the first element in his success. The next was that on his return to America he got from the Harvard Observatory—that storehouse of astronomical facts—the spectral types of his stars; and combining these with the measures of distance (which told him the intrinsic brightness or luminosities of the stars) he found that stars of the same spectral type M fell into two distinct groups separated by an interval. There were very bright stars, now called Giants, and there were very faint ones, now called Dwarfs, but none of intermediate stature.

The same was true in minor degree for stars of other types, but as the B end of the series was approached the gap gradually disappeared much in the way that the gap between the legs of a step-ladder gradually lessens as we approach the top. Indeed Russell's diagram of his results is very like a step-ladder, the top representing the B stars followed by A, F, G, K, M, in descending order, and the gap between the two legs of the ladder representing the difference in luminosity, as the intrinsic brightness of a star has come to be called. Russell brought this diagram with him when he came to attend the meeting of the Solar Union at Bonn in 1913. It is sad to remember the occasion, for the most friendly relations seemed to have been permanently established between the various nations assembled. We remember with especial regret the trip on a great steamer on the Rhine which ended the meeting, and, alas! was the end also of our hopes of a permanent friendliness, for before the

I cannot give you a better idea of the value of Professor Eddington's work than by quoting a few words from a letter written to me by Mr. Russell, again specially in response to a request mentioning this lecture :—

"What appeals to me as the big thing is Eddington's work on radiative equilibrium (MN **77**, p. 16 and p. 596). The importance of this can hardly be exaggerated ; it is not too much to say that it is the first rational theory of stellar constitution."

Eddington has in fact given us a rough attempt at tracing the history of a star of given mass. By way of illustration let us consider our own sun. He is now a "dwarf star," on the descending leg of the ladder, of spectral type G, and with a surface temperature of about 5,000° C., and an absolute magnitude 5·1. Looking back into the past he was at one time much hotter and of type F, and probably never rose much higher than this on the ladder. Before that his history lay on the ascending leg, and there was a time when his spectral type was just as at present, but his absolute magnitude was near zero, five magnitudes greater than at present. This means that the total light was 100 times greater than now, and since the surface was in a similar radiative state, it must have been 100 times more extensive. The diameter of the sun was therefore ten times the present diameter—ten million miles instead of one million. Where our little earth may have been at that time we can scarcely conjecture, but supposing for a moment that we had been able to regard the sun in our present conditions, he would have taken nearly an hour to rise instead of a few minutes; and when risen, his disc would be ten times as great in all directions—a "giant" sun indeed! And yet this magnification of 10 to 1 is only modest compared with the extreme possibilities.

We set out by the recollection of Jack the Giant Killer, but our road has led us rather to think perhaps of Jack and the Beanstalk. We have climbed up to Giant Land, the land of the giant suns, not by a beanstalk, but by means of the trembling rays of light, a ladder which does not grow upwards from our earth, but is let down to it by the giants themselves. "Fee Fo Fum!" said the Giant, "I smell the blood of an Englishman." In our analogy the giants have been invaded, not by an Englishman, but chiefly by an American ; but at any rate we have the satisfaction of reflecting that his work began in Cambridge when he was a student, and that at the end of it there has emanated from Cambridge this brilliant confirmation by Professor Eddington of which the discoverer has himself expressed such generous appreciation.

[H. H. T.]

GENERAL MONTHLY MEETING,

Monday, February 3, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Pursuant to Bye Laws, Chap. IV. Art. 2, Arthur James Walter, Esq., K.C. LL.B., was elected a Manager, to succeed Sir Charles Norris Nicholson, Bart., M.P. M.A. LL.B., deceased.

The Special Thanks of the Members were returned to "An Old Member" for a donation of £50 to the General Fund of the Institution, in celebration of fifty years of membership.

The Treasurer reported that the Institution had received a legacy of £300 from the Executors of the late Dr. T. Lambert Mears, who was a Member for fifty-three years.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Bibliography of Indian Geology and Physical Geography, 1917-1918. 8vo.
Geological Survey Records, Vol. XLIX. Part 2. 8vo. 1918.
Scientific Report of Agricultural Research Institute, Pusa, 1917-18. 8vo. 1918.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta, Rendiconti: Classe di Scienze Fisiche, Matematiche e Naturali, Vol. XXVII. 2° Sem. Fasc. 7-10; Classe di Scienze Morali, Vol. XXVII. 1° Sem. Fasc. 3-4. 1918. 8vo.
American Geographical Society—Geographical Review, Nov. 1918. 8vo.
American Philosophical Society—Proceedings, Vol. LVII. No. 6. 1918. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXIX. No. 1, Nov. 1918. 8vo.
Bankers, Institute of—Journal, Vol. XL. Part 1, Jan. 1919. 8vo.
Batavia, Royal Magnetical and Meteorological Observatory—Observations, Vol. XXXVII. 1914. 4to. 1918.
Observations, Secondary Stations, Vol. IV.-V. 1914-15. 4to. 1917-18.
Birmingham and Midland Institute—Report, 1918. 8vo. 1919.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXVI. Nos. 2-3. 1918. 4to.
British Astronomical Association—Journal, Vol. XXIX. No. 2. 1919. 8vo.
Buenos Aires—Bulletin of Statistics, July-Aug. 1918. 4to.

year had passed the Great War had shattered them all. It was on his return from Germany through England that Russell showed us his step-ladder diagram at the Royal Astronomical Society, and expounded his views on the evolution of a star, which were that its life began at the foot of the upright leg, the ascent of which signified that the star was growing continually hotter and changing its spectral type meantime from M upwards towards B, that at B the increase of temperature was arrested and after a time cooling began carrying the star down the inclined leg of the ladder through changes in the reverse order. The only weak spot in the evidence arose from the small number of observations. To determine the actual or intrinsic brightness of a star we must know its distance, and there are not many stars of which the distance can be easily measured, and though Russell had himself increased the number, the total was still not large. To get further evidence he had recourse to indirect estimates of distance, especially those of clusters of stars. We have lately become more and more aware of the association of stars in clusters represented by their common movement, somewhat in the way that the movements of a flock of birds migrating from one place to another are associated. If we may accept this evidence, and if we can determine the distance of any one star in the cluster, the distances of the others can be inferred. In Russell's skilful hands this evidence was collated and found to strengthen his conclusions.

Let us pause here for a moment to reflect on the inherent probability of the suggestion. Is it not after all much more likely that a star first rises in temperature and then falls rather than that it should be permanently either rising and falling? Now that the idea has been put forward, and that there seems to be not only good evidence of this change in the sky, but, as we shall presently see, also good theoretical reason for it, we wonder why the idea was not the most natural one to adopt from the first. But curiously enough it was not the one adopted by astronomers, with the notable exception of Sir Norman Lockyer, who made the same suggestion as Russell's (though on different grounds) many years before. May I give a crude illustration from our ordinary life of the mistake that was made by many of us? It is as though we had taken the amount of hair as an indication of the age of a man. In very early life the amount of hair is small, it increases with age up to a certain point, but then it begins to decrease until a very old man often has as little hair as a new-born baby. We could give Shakespeare's Seven Ages of Man according to the amount of hair in the same diagrammatic form as Russell's step-ladder, beginning with the baby at the foot of the upright leg, ascending to the man in middle life with maximum hair (corresponding to the maximum temperature), and placing the greater ages down the inclined leg till we arrive again at a bald pate. Shakespeare reminds us with his phrase about the voice "turning again toward a childish treble" that not only the hair but the voice goes through changes which show a reversal after middle life. We were practically confusing the baby stars with the old man stars until Russell called our attention to the fact; and now it seems quite easy to make the distinction. But there was some hesitation before the new views were accepted at all, chiefly on account of the lack of sufficient measures of distance, which left room for doubt. Recently the evidence has been reinforced in a remarkable way by a totally new and unexpected method for inferring the distances of stars, due to Mr. W. S. Adams, of the Mount Wilson Observatory in California. His discovery is that if we have two stars, one of which is very bright intrinsically and the other faint, but both of the same spectral type, we can find two lines of the spectra which have different relative intensities: let us call them A and B. In the bright star A is more intense than B, in the faint star B will be more intense than A. Now observe that this difference will persist however far we may remove the stars from us. By altering the distances we may make the brighter star *appear* the fainter, but we can pierce its disguise by noting simply that the line A in the spectra is the more intense, so that if the star *appears* faint we see at once that this must be due to its greater distance. In fact we can infer the distance from the relative intensities of the lines A and B, so that Adams has really given us a new method of inferring distances. The new method has the further advantage of requiring far less labour than the old method of parallax; in fact, when once the spectrum has been photographed the further labour required is quite small, so that by this time Adams has been able to give us the luminosity of hundreds of new stars, and by this overwhelming evidence confirms Russell's results derived from merely a few. In reply to a request he has sent me specially for this lecture the following table of results, and I am sure you will appreciate his kindness. [In the lecture the results were represented diagrammatically; here Adams's actual figures are reproduced on the next page. To see the "step-ladder" hold the table sideways, so that the column of absolute magnitudes is at the bottom. The upright leg is then represented by the two rules across the page. It will be seen that the majority of intrinsically luminous stars are contained between these lines. The sloping leg is easily seen from the lie of the figures. That there are very few faint stars of class M does not mean that there are few in the heavens, but that they are the most difficult to observe from their faintness.]

In addition to this confirmation by new observations we have had an independent confirmation by the brilliant theoretical work of Professor Eddington, who has attacked the problem of the life of a star mathematically. He supposed a mass of gas first of all to be simply under the action of its own gravity. It will consequently contract, and owing to the contraction will rise in temperature; but Professor Eddington soon found that this simple hypothesis would

W. S. ADAMS'S RESULTS UP TO NOVEMBER 8, 1918.

Abs. Mag.	Mc to M*d*	K_4 to K_9	G_9 to K_3	F_9 to G_8	A_7 to F_8
− 2·8	1	...	...	...	1
− 2·3	1	...	...	...	2
− 1·8	1	...	...	2	3
− 1·3	1	4	1	3	2
− 0·8	15	34	12	7	5
− 0·3	11	36	33	33	7
+ 0·2	35	11	70	44	3
+ 0·7	13	...	72	34	12
+ 1·2	6	...	22	21	13
+ 1·7	1	...	12	8	5
+ 2·2	...	...	7	2	13
+ 2·7	...	...	3	5	15
+ 3·2	...	...	4	2	17
+ 3·7	...	...	1	7	20
+ 4·2	...	...	3	11	35
+ 4·7	...	...	3	28	22
+ 5·2	...	1	4	21	18
+ 5·7	...	0	8	26	...
+ 6·2	...	3	38	32	...
+ 6·7	...	11	15	4	...
+ 7·2	...	13	10	...	...
+ 7·7	...	5	3	...	...
+ 8·2	...	8	...	...	...
+ 8·7	...	13	...	...	...
+ 9·2	...	4	...	...	...
+ 9·7	3	2	...	...	...
+10·2	4	...	...	...	...
+10·7	3	...	...	...	...
+11·2	3	...	...	...	...
+11·7	...	...	...	...	...
+12·2	...	...	...	...	...
+12·7	...	...	...	...	...
+13·2	1	...	...	...	...
Total ...	99	145	321	290	193

not answer—it led him to impossible results. Clearly something else besides gravity must be at work, and he was driven to the further hypothesis that the *radiation-pressure* inside the star played an important part in its history. Radiation-pressure (or if we like to call it so, light-pressure) is what makes the tail of a comet. As a comet approaches the sun it begins to feel the effects of the fierce light, which is known to be able to drive away very small particles from the head of the comet, much as we can blow away chaff from wheat. In consequence of this action the small dust-like particles which may exist in the head are believed to be driven outwards to form the tail. But this force is not merely in existence on the outside of the sun, it permeates its whole body. A particle inside the sun is of course receiving radiation-pressure from all its surroundings, but the pressure will naturally be greater on the hotter side, i.e. on the side of the sun's centre. Working out the problem afresh with the addition of this new factor, Professor Eddington has obtained results which agree satisfactorily with the observed effects, and indeed the closeness of the agreement is startling. He is able to utilise the fact noticed earlier in the lecture, that the masses of the stars are not very different, so that it is easy to take three representative cases—let us say one in which the mass is equal to that on our sun, one in which it is 5 times greater, and one in which it is 5 times less—and by following these three cases in detail he can show the distinctive features of different stars. Briefly, the step-ladder is highest for the star of greatest mass, which may get hotter and hotter until it reaches type O; a star of intermediate mass like our sun is arrested at a lower height and may not reach higher than type F, or at best A, before it begins to fall down the inclined leg; while a star of small mass may reach no higher than type K at any time. The golfers in the audience may be reminded of their handicaps. Those who are destined to be scratch players (probably, however, *not* because of their great mass) improve very rapidly until they reach the highest pitch of excellence, and it may even be only in old age that they begin to travel downwards; but then there are others of long handicap, who although they may improve a little at first never get beyond the fatal 18 at their best, and on whom declining years soon begin to leave their mark.

One of the most remarkable suggestions of Professor Eddington's work gives a reason for the close resemblance in mass of the stars. There is a certain mass for which the radiation-pressure pressing outwards nearly balances the force of gravitation pulling inwards, and it is clear that for stars as large or larger than this a break-up sooner or later is to be expected. This assigns very obviously the upper limit to the masses—we can easily see why there are no stars larger than a certain limit. But how about the lower limit? Are there no stars *very* much smaller than this? Certainly there are: we are living on one of them. Our earth is smaller by some thousands of times; but then it is not a star in the full sense, for it is not shining with its own light. If it did ever so shine the light must have been feeble at best and have only lasted for a very short time. There may in fact be many small stars, but *we do not see them*, and accordingly have not reckoned them in saying that the masses of the stars are closely similar.*

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* On reading this again I realise that it does not do full justice to Eddington's suggestion for the lower limit. He shows a definite difficulty in the formation of small stars.

I cannot give you a better idea of the value of Professor Eddington's work than by quoting a few words from a letter written to me by Mr. Russell, again specially in response to a request mentioning this lecture :—

"What appeals to me as the big thing is Eddington's work on radiative equilibrium (MN **77**, p. 16 and p. 596). The importance of this can hardly be exaggerated; it is not too much to say that it is the first rational theory of stellar constitution."

Eddington has in fact given us a rough attempt at tracing the history of a star of given mass. By way of illustration let us consider our own sun. He is now a "dwarf star," on the descending leg of the ladder, of spectral type G, and with a surface temperature of about 5,000° C., and an absolute magnitude 5·1. Looking back into the past he was at one time much hotter and of type F, and probably never rose much higher than this on the ladder. Before that his history lay on the ascending leg, and there was a time when his spectral type was just as at present, but his absolute magnitude was near zero, five magnitudes greater than at present. This means that the total light was 100 times greater than now, and since the surface was in a similar radiative state, it must have been 100 times more extensive. The diameter of the sun was therefore ten times the present diameter—ten million miles instead of one million. Where our little earth may have been at that time we can scarcely conjecture, but supposing for a moment that we had been able to regard the sun in our present conditions, he would have taken nearly an hour to rise instead of a few minutes; and when risen, his disc would be ten times as great in all directions—a "giant" sun indeed! And yet this magnification of 10 to 1 is only modest compared with the extreme possibilities.

We set out by the recollection of Jack the Giant Killer, but our road has led us rather to think perhaps of Jack and the Beanstalk. We have climbed up to Giant Land, the land of the giant suns, not by a beanstalk, but by means of the trembling rays of light, a ladder which does not grow upwards from our earth, but is let down to it by the giants themselves. "Fee Fo Fum!" said the Giant, "I smell the blood of an Englishman." In our analogy the giants have been invaded, not by an Englishman, but chiefly by an American; but at any rate we have the satisfaction of reflecting that his work began in Cambridge when he was a student, and that at the end of it there has emanated from Cambridge this brilliant confirmation by Professor Eddington of which the discoverer has himself expressed such generous appreciation.

[H. H. T.]

GENERAL MONTHLY MEETING,

Monday, February 3, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Pursuant to Bye Laws, Chap. IV. Art. 2, Arthur James Walter, Esq., K.C. LL.B., was elected a Manager, to succeed Sir Charles Norris Nicholson, Bart., M.P. M.A. LL.B., deceased.

The Special Thanks of the Members were returned to "An Old Member" for a donation of £50 to the General Fund of the Institution, in celebration of fifty years of membership.

The Treasurer reported that the Institution had received a legacy of £300 from the Executors of the late Dr. T. Lambert Mears, who was a Member for fifty-three years.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Bibliography of Indian Geology and Physical Geography, 1917-1918. 8vo.
Geological Survey Records, Vol. XLIX. Part 2. 8vo. 1918.
Scientific Report of Agricultural Research Institute, Pusa, 1917-18. 8vo. 1918.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta, Rendiconti: Classe di Scienze Fisiche, Matematiche e Naturali, Vol. XXVII. 2° Sem. Fasc. 7-10; Classe di Scienze Morali, Vol. XXVII. 1° Sem. Fasc. 3-4. 1918. 8vo.

American Geographical Society—Geographical Review, Nov. 1918. 8vo.

American Philosophical Society—Proceedings, Vol. LVII. No. 6. 1918. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LXXIX. No. 1, Nov. 1918. 8vo.

Bankers, Institute of—Journal, Vol. XL. Part 1, Jan. 1919. 8vo.

Batavia, Royal Magnetical and Meteorological Observatory—Observations, Vol. XXXVII. 1914. 4to. 1918.
Observations, Secondary Stations, Vol. IV.-V. 1914-15. 4to. 1917-18.

Birmingham and Midland Institute—Report, 1918. 8vo. 1919.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXVI. Nos. 2-3. 1918. 4to.

British Astronomical Association—Journal, Vol. XXIX. No. 2. 1919. 8vo.

Buenos Aires—Bulletin of Statistics, July-Aug. 1918. 4to.

Canada, Department of Mines—Geological Survey: **Memoirs, No. 82, 1915.** 8vo. 1918.
Summary Report for Year ending Dec. 31, 1917. 8vo. 1918.
Museum Bulletin, No. 28. 8vo. 1918.
Canada, Royal Society of—Transactions, Series III. Vol. XI. June-Sept., Dec. 1917, March 1918. 8vo.
List of Officers and Minutes of Proceedings, Vol. XI. 8vo. 1917.
Carnegie Institute of Washington—Contributions from Mount Wilson Solar Observatory, Nos. 147-149, 151-157. 8vo. 1918.
Chemical Industry, Society of—Journal, Dec. 1918–Jan. 1919. 8vo.
Chemical Society—Journal for Dec. 1918-Jan. 1919. 8vo.
Proceedings, Dec. 1918-Jan. 1919. 8vo.
Editors—Aeronautical Journal for Nov. 1918. 8vo.
Athenæum for Jan. 1919. 4to.
Author for Jan. 1919. 8vo.
Chemist and Druggist for Jan. 1919. 8vo.
Church Gazette for Jan. 1919. 8vo.
Dyer and Calico Printer for Jan. 1919. 4to.
Electrical Times for Dec. 1918-Jan. 1919. 4to.
Engineer for Dec. 1918-Jan. 1919. fol.
Engineering for 1918. fol.
Horological Journal for Jan. 1919. 8vo.
Journal of Physical Chemistry for Dec. 1918. 8vo.
Junior Mechanics for Jan. 1919. 8vo.
Law Journal for Jan. 1919. 8vo.
London University Gazette for Dec. 1918-Jan. 1919. 4to.
Model Engineer for Dec. 1918-Jan. 1919. 8vo.
Musical Times for Nov.-Dec. 1918-Jan. 1919. 8vo.
Nature for Dec. 1918-Jan. 1919. 4to.
New Church Magazine for Jan.-Feb. 1919. 8vo.
Nuovo Cimento for May-June 1918. 8vo.
Science Abstracts for Nov.-Dec. 1918. 8vo.
Wireless World for Jan. 1919. 8vo.
Zoophilist for Jan. 1919. 8vo.
Faulds, H., Esq. (the Author)—The Hidden Hand: A Contribution to the History of Fingerprints. 8vo. 1917.
Firenze, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani. No 209, Sept.-Oct. 1918. 8vo.
Franklin Institute—Journal, Dec. 1918. 8vo.
Geneva, Société de Physique—Comptes Rendus des Séances, Vol. XXXV. No. 3. 8vo. 1918.
Geographical Society, Royal—Journal for Dec. 1918-Jan. 1919. 8vo.
Geological Society of London—Abstracts of Proceedings, Nos. 1030-1032. 8vo. 1919.
Quarterly Journal, Vol. LXXIII. Part 4. 8vo. 1918.
Imperial Institute—Bulletin, Vol. XVI. No. 3. 8vo. 1918.
Indian Association for the Cultivation of Science—Proceedings, Vol. IV. Parts 1-2. 8vo. 1918.
Linnean Society—Journal, Zoology, Vol. XXXIV. No. 226. 8vo. 1919.
London County Council—Gazette for Jan. 1919. 4to.
London Society—Journal for Jan. 1919. 8vo.
Meteorological Office—Daily Readings for Oct. 1918. 4to.
Geophysical Journal—Daily Values for Dec. 1918. 4to.
Monaco, Musée Océanographique—Bulletin, Nos. 348-349. 8vo. 1918.
New York, Society for Experimental Biology—Proceedings, Vol. XVI. No. 1. 8vo. 1919.
New Zealand, High Commissioner for—Census, 1916, **Part 3. Appendix E.** 4to. 1918.
Statistics for Year 1917, Vols. I.-II. 4to. 1918.

Nizamiah Observatory—Astrographic Catalogue, 1900 (Hyderabad Section), Vol. II. 4to. 1918.

Pharmaceutical Society of Great Britain—Journal for Dec. 1918–Jan. 1919. 8vo.

Photographic Society, Royal—Journal for Dec. 1918–Jan. 1919. 8vo.

Post Office Electrical Engineers—Journal, Vol. II. Part 4. 8vo. 1919.

W. E. Co.'s Automatic Telephone System. By B. O. Anson. 8vo. 1918.

Rockefeller Institute for Medical Research—Studies, Index, Vols. I.–XXV. 8vo. 1918.

Royal Engineers' Institute—Journal, Vol. XXIX. No. 1. 8vo. 1919.

Royal Society of Arts—Journal for Jan. 1919. 8vo.

Royal Society of London—Philosophical Transactions, A, Vol. CCXVII. No. 550. 4to. 1918.

Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXV. No. 1. 8vo. 1919.

Scottish Meteorological Society—Journal, Vol. XVIII. No. 25. 8vo. 1918.

Smithsonian Institution—Miscellaneous Collections, Vol. LXVIII. Nos. 9, 11–12; Vol. LXIX. Nos. 2–4, 7–8. 1918. 8vo.

Società degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. VII. Disp. 7–9. 4to. 1918.

Swinton, Alan A. Campbell, Esq., F.R.S., M.R.I. (*the Author*)—Science and the Future. An Address delivered at the Opening Meeting of the Royal Society of Arts. 8vo. 1918.

Tôhoku Imperial University, Sendai, Japan—Science Reports, 2nd Series, Vol. V. No. 1. 4to. 1918.

Mathematical Journal, Nov. 1918. 8vo.

Tokyo, Imperial Academy—Proceedings, Vol. I. No. 5. 8vo. 1918.

United States Department of Agriculture—Journal of Agricultural Research, Nov. 1918. 8vo.

Experiment Station Record, Vol. XXXVIII. Index No.; Vol. XXXIX. Nos. 3–5. 1918. 8vo.

United States Naval Observatory—Publications, 2nd Series, Vol. IX. 4to. 1918.

United States Patent Office—Official Gazette, Vol. CCLII. No. 2—Vol. CCVI. No. 4. 8vo. 1918.

Washington, National Academy of Sciences—Proceedings, Vol. IV. No. 11. 8vo. 1918.

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WEEKLY EVENING MEETING,

Friday, February 7, 1919.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Colonel George Adami, M.D. F.R.S., Canadian Army Medical Corps.

Medicine and the War.

Now that close upon three months have elapsed since that memorable eleventh hour of the eleventh day of the eleventh month of 1918 when the guns ceased, the time is ripe to view an aspect of the war which interests all thoughtful people—namely, the part played by medicine and medical advance in the prosecution of the campaign. Your good physician in times of peace does not vaunt his wares, and when he dons khaki his spirit does not alter. As a result, throughout the war little that is authoritative has been published regarding the broad aspects of the work of the medical units. Correspondent after correspondent, it is true, writing from the war zone has borne witness to the wonderful way in which the army medical service has risen to the occasion, to its capability, flexibility and devotion, but this is almost the first occasion that has offered itself for a general review of the outstanding accomplishments of the service, and fortunately my semi-detached position as an officer, not of the Royal Army Medical Corps, but of the Canadian Expeditionary Force, permits me to speak openly and without false modesty regarding the good deeds and great achievements of my British colleagues. It is but right and fitting that the people of the Empire should realise the extent of the victory and of their debt to the R.A.M.C., for truly the conquest of disease has been the greatest and the noblest victory of all.

Here, indeed, the first difficulty presents itself. So great has been the victory, so many the advances, that it is impossible to deal adequately with my topic in the course of one hour. I cannot give the complete picture, at most a rapid sketch with the high lights dashed in.

Let me in the first place begin by erasing the generally accepted picture. What has impressed the war correspondent has been the triumph of the surgeon in the care of the wounded. The general opinion, even in the profession, both before and during the war, has been that the outstanding advances of the generation have been the surgical. Now at the risk of being thought paradoxical, without in the least wishing to minimise the great work accomplished by the army surgeon, I would lay down that *during the war surgery has played but a secondary part.* An actor of the first rank billed to perform a secondary part may enact that part so admirably as to put the leading characters into the shade. That has been the case with our war surgeons. Their work has been of the best; they have achieved notable advances, particularly under the inspiration of General Sir Robert Jones, in the treatment and after-treatment of fractures and wounds of the extremities. The amount of crippling has been reduced to a very remarkable degree. But this must be said: the wounded appeal to the onlookers much more than do the victims, say, of pneumonia and dysentery. The man whose honourable scars remain as an ever-present reminder of the victory of the surgeon over death brings down the house in a way that the soldier stricken by some dread fever and restored to health can never hope to emulate. The hundreds of thousands who through the sanitarian have never been ill at all make no impression whatsoever upon the public: their sound health is taken for granted: it is an evidence of their good physique; *they* are to be congratulated, not the physicians who, controlling their surroundings, have saved them from illness.

This brings me to my point. The great, the outstanding feature of the war has been the triumph of preventive medicine. It is that triumph, and that triumph alone, that has made possible our eventual success.

For look at the matter dispassionately. While a grateful country owes to the sick or wounded soldier every care, each hospital case weakens the army, not merely by the loss of an individual from the front line, but by the diversion of others from active soldiering to purposes of transport, orderly work, and administration services in ambulance, hospital, hospital trains, and so on. Throughout the centuries campaign after campaign has been brought to nothing or to an end by pestilence. Not le général Fevrier but General Le Fièvre was the great leader of the hosts of death and the ultimate victor. Had our sickness and death rolls been what they were at the beginning of the century, in the Boer War, the results of this campaign would have been very different.

You will understand, therefore, why it is that in attempting this review I place first what I may term preventive pathology—the research into the cause of disease which must precede the scientific application of methods of prevention, based upon these researches and their outcome. Let me give you a few outstanding examples of what has been accomplished.

THE CONTINUED FEVERS.

To-day we all speak glibly of typhoid fever, and even of paratyphoid A and paratyphoid B, of typhus, relapsing fever, and malaria—so glibly that it is difficult to realise that the sure differentiation and recognition of these distinct diseases has occurred during the lifetime of some here present. Well-marked cases of malaria had, it is true, been distinguished clinically for some generations by the shivering and hot stages of the ague and the swollen spleen or "ague cake." But until our generation there was no means of making a sure diagnosis, and in the war between the North and the South in the States in the "sixties," and in the Boer War only a score of years ago, it was impossible to determine whether an important group of cases was either typhoid or malaria, or both, and they were described in the returns as typho-malaria. It has been through medical research in our generation that a sure diagnosis has been made possible.

When, on the one hand, Laveran, the great French pharmacist and parasitologist, discovered the organism of malaria in the blood, and, on the other, Eberth and Gaffky showed how to isolate and recognise the organism of typhoid fever, and Durham, Gruber and Widal showed how the blood of a typhoid fever patient agglutinated the Eberth-Gaffky bacillus—then, when these observations were elevated into routine clinical methods with which every medical officer must be acquainted—then, at last, diagnosis of these diseases became easy. It has been by this routine observation that the paratyphoid fevers have been differentiated from ordinary typhoid, by the routine use of the microscope in the clinical laboratory that cases of relapsing fever can be made out, and by clinical study and negative laboratory findings that cases of typhus are differentiated from the other fevers.

It is these so-called continued fevers that have been the great bane of armies of the past. Let us see what has happened in this war.

Typhoid.—Let us first take enteric or typhoid fever. In South Africa, but twenty years ago, with a total force of something over half-a-million men, 129·9 out of every thousand men were admitted to hospital with the diagnosis of enteric, as compared with 47·95 admitted with wounds. In other words, of our British troops one man out of every eight went down with typhoid. One quarter of all admissions was from this one disease, and of them 18·6 out of every 1000 died, as compared with 2·9 who died of their wounds and 9·59 killed in action. Or, twice as many men died of typhoid as were killed in action, and six times as many as died as the result of wounds.

With these figures let me compare those of the present war. I select those for the Canadian overseas forces, and that because in the first place they are the more easily available to me. These are, I gather, better than those for the British forces as a whole, and that because Canada showed a greater sympathy for the well-being of her troops as a whole, and no misplaced sympathy for the stupid conscientious objector. If a man volunteered to fight but objected to inoculation, his services for overseas were declined. He was not allowed to endanger the health of his comrades. It is interesting to note that one battalion alone, the first to cross, and that before routine inoculation against typhoid was fully established, was only inoculated in part, and that battalion afforded many more cases than any other. I owe the figures to my chief, Surgeon-General G. La F. Foster, C.A.M.C., and more particularly to Lieut.-Colonel F. G. Bell, C.A.M.C., A.D.M.S. in charge of Hospitalisation.

In the second place the two sets of figures are well adapted for comparison. The numbers engaged are roughly equal, 548,237 being the number engaged in the Boer War, 420,000 odd the number of Canadian troops who came overseas. The Boer War lasted 31 months; the first Canadian contingent arrived in England in October 1914, and thus the period of exposure was 49 months.

Compared with the 59,684 admissions to hospital with typhoid in South Africa, there were 412 Canadian admissions for this disease. In place of 8,248 deaths (excluding officers) there have been altogether (including officers) 14! Indeed, out of 10,000 young officers of the Canadian Expeditionary Force, men at the most susceptible age for typhoid, i.e. between 18 and 30, not one who came overseas died of the disease.

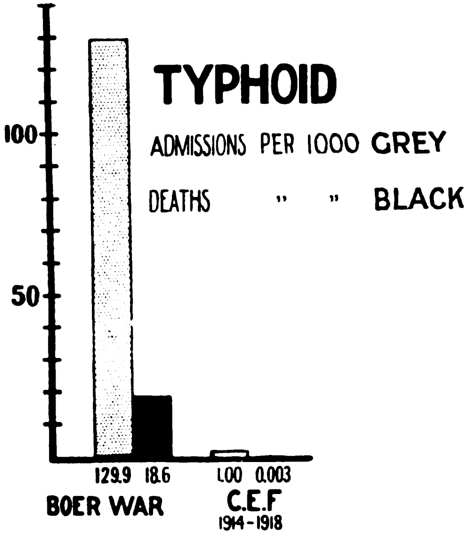

FIG. 1.

I owe to the courtesy of Lieut.-General Sir Thomas Goodwin, D.G.A.M.S., and to Brig.-General Sir William Leishman, the following data. Among the Imperial troops, steadily through the war, the number of those who submitted themselves to inoculation had increased, until now no less than 97 per cent have been thus protected. Although it is true that some 11 per cent of those have not had re-inoculation during the last twelve months, the number of admissions to hospital from enteric fever has been reduced to the extraordinarily low figure of 1·5 per thousand, as compared with 1 per thousand among the Canadians; and as regards the tens of thousands of officers, there was only one death in 1916 and one in 1917. As regards the inoculated and the uninoculated, roughly, in proportion to the numbers, there were ten times as many uninoculated admitted to hospital as inoculated.

With all due deference to the Army surgeons, and their work, they can show no triumph comparable with this.

Had the same ratio of enteric cases been maintained as in the Boer War, our Canadian Director-General would have had to provide seven more General Hospitals, of 1,040 beds each, throughout the war, some 200 more medical officers, and 500 more nursing sisters. Imagine what would have been the corresponding additional demand for the British Army with its millions in place of the Canadian hundred thousands.

What is the meaning of these most striking facts? They mean that, thanks to medical research, we have learnt how to render troops immune to typhoid fever. It does not mean that they were not exposed to infection. Far from it. Flanders with its sodden countryside and stagnant waterways everywhere was rife with the disease; there were 2,000 deaths from typhoid that first winter among the inhabitants among whom our troops were quartered. The German and the French troops, not properly inoculated, or uninoculated, suffered heavily until they too learnt the lesson and followed our methods, and we owe our protection very largely to the work of the R.A.M.C.

An able Russian working in India, Dr. Haffkine, had shown that protection could be given against cholera by inoculating individuals with killed cultures of the cholera spirillum. At the time of the Boer War Sir Almroth Wright, then Professor of Pathology at the Army Medical College at Netley, elaborated a similar typhoid vaccine, and obtained encouraging but not wholly satisfactory results. It was left to his successor Colonel, now General, Sir William Leishman to improve Sir Almroth's methods, and elaborate a vaccine and technique of administration that was thoroughly efficient, so effective that long months before the war it was adopted for the U.S. Army, in which inoculations were made obligatory. Fortunately for Canada, in the spring before the war, and at the suggestion of the late Director-General, Colonel, now General, Carleton Jones, Sir

William Leishman had visited Canada, had lectured and demonstrated his methods, and had so convinced those in authority that the enforcement of inoculation for all overseas troops was an easy task.

When cases of the paratyphoid diseases showed themselves, by giving combined inoculations of killed cultures of all three specific organisms, our troops were protected during the course of the war against typhoid, paratyphoid A and paratyphoid B. This combined inoculation, if I mistake not, was first started in the Army at the instigation of my colleague, Major L. J. Rhea, C.A.M.C., among the Montreal and Quebec troops early in 1915.

Typhus.—Equally great has been the triumph over another of the great war pestilences. By good fortune, coupled with good sanitation and good feeding, this did not affect our own troops, but it exacted a terrible toll from our brave allies, the Serbs, after they had repulsed the Austrians and gained possession once again, for a time, of their devastated country. War, pestilence and famine coming together mean typhus, accompanied often by relapsing fever. The researches more particularly of Ricketts, the American, who fell a victim to the disease, upon "tabardillo" or Mexican typhus, and of Dutton and Todd upon an African relapsing fever, indicated very strongly that these two diseases are conveyed from individual to individual by body parasites, and, as our troops learned from bitter personal experience, prominent among these is the body louse. When the disease was at its height the Serbians appealed to the British Government for medical help, and the War Office sent a Commission under Colonel, now General, William Hunter. It is a most fascinating story; how army and people placed themselves unreservedly under Hunter's control; how, acting in accordance with these indications, Hunter disinfected a whole people, stopped railway travel for a time so that the infected might not mingle with the disinfected, until the whole country had been treated; how he converted railway vans into simple and effective disinfecting chambers; how he steam-heated all the clothing and bedding; and how in a few weeks the epidemic was almost absolutely arrested. We did not employ equally drastic methods in France, and as a consequence, until the last few months, it was only certain Divisions that were drastic in their methods that, like the Guards and the Canadians, were effectively deloused.

Tetanus.—Next to report another victory :—

When Pasteur was making his classical experiments upon anthrax in cattle, and was engaged in demonstrating—just about the same time as Darwin was writing his book upon the earth-worm—that these worms convey the spores of the anthrax bacillus from the bodies of infected animals buried in the ground up to the surface (thereby explaining how years later other animals became infected), he noted that while some of the rabbits inoculated under the skin with worm-casts died of anthrax, others died of tetanus. Evidently the germs of this disease existed in field and garden soil. It was left to Nicolaier, in Germany, to recognise the presence of the characteristic drum-stick bacilli—bacilli with a spherical spore at one end—and to the great Japanese bacteriologist Kitasato, to obtain these bacilli in pure culture, and with the pure cultures to reproduce the disease in the lower animals.

They are remarkable bacilli. Pasteur was the first to show that there exist microbes which can live and multiply in the absence of air and free oxygen, a state of affairs which was thought unbelievable, oxygen being regarded as necessary for life. So it is, but not necessarily free oxygen as it is in the air. These "anaerobes" prefer to obtain what oxygen they want by taking it up in a combined form from their foodstuffs—from sugars, for example, and proteins—and the tetanus bacilli are anaerobes. As a consequence they will not grow in surface wounds. They need deep wounds such as those made by projectiles, or wounds with pockets. As to these pockets, and how they help the bacillus to grow, Sir Almroth Wright, working for the Medical Research Committee, has made some exquisite observations during the war. It was the leading American bacteriologist, Dr. Theobald Smith, of Boston, who first observed that anaerobes could be made to grow under not strictly airless conditions. He showed that if a small piece of fresh meat, or liver, or kidney be dropped into a tube of broth exposed to the air, and this was then inoculated with the bacilli, they grow in and upon the meat. Wright showed that the same growth occurred if one took a fine capillary tube, filled this with broth containing the bacilli, and dropped it into an ordinary large tube of air-containing broth. Confined in the capillary tube to which the oxygen from the surrounding broth could not gain free access, the bacilli multiplied freely. Evidently the dead tissue of wounds fixes the oxygen in its immediate neighbourhood, and protected thus the bacilli can grow.

In the States, until quite recently, the crop of cases of lockjaw following 4th of July celebrations was simply appalling. There were more lives lost each year from this cause than in the Spanish-American War. Countless "kiddies" stayed up at night to set off fireworks and crackers in the gardens and streets; let them explode in their hands begrimed with street or garden earth, and sure enough in a week or two lockjaw set in. Since 1910 or so, "Fourth of July tetanus" has been largely eradicated, and that because everywhere throughout the States tetanus anti-serum or anti-toxin is kept in stock, and the surgeons have been directed in every case of 4th of July wounds to inject immediately an adequate dose, in this copying a procedure originated, if my memory serves me aright, in France in regions where tetanus is common.

What is this anti-serum? Well, it is of the same nature as diphtheria anti-toxin. It has been shown that if we take a horse and, beginning with non-fatal, give it progressively larger and larger doses of the broth in which tetanus bacilli have been grown, and into which they have discharged their most potent poisons or toxins, we can render these horses immune to tetanus, so that eventually they will stand doses of the living bacilli and their toxins sufficient to kill a thousand horses, and having thus raised their immunity to a very high degree, the fluid of their blood (their blood serum) is found to neutralise the tetanus poison, so that injected into an animal along with several times the fatal dose of tetanus toxin, or along with the actual bacilli, nothing happens—or rather the animal shows no ill effects. Lockjaw has for long years been so rare in civil practice in England that we were wholly unprepared in 1914 to encounter it as

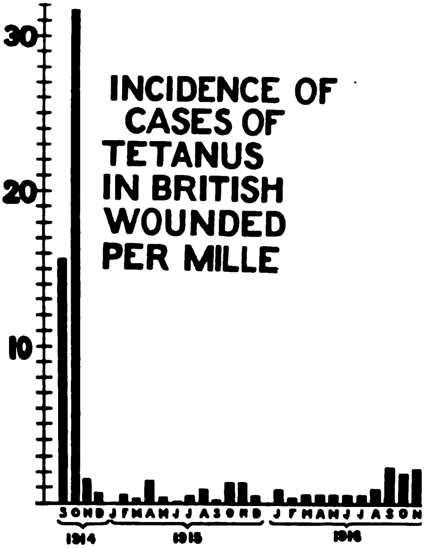

Fig. 2.

a frequent condition among the wounded, the more so as it was almost unknown in the South African War. But the soil on the veldt is very different from that in the highly cultivated and highly manured districts of France. When in 1914 our troops got down to the Marne, and still more later in the Aisne area, they fought over ground that had been cultivated for long generations. Presently case after case of those wounded in this area and transferred to the general hospitals on the French sea-board, or to England, manifested this terrible and most fatal complication. I should explain that it is days and often weeks before it shows itself, and mercifully this long incubation period gives us our opportunity to protect the individual.

We had not expected it, and we were unprepared. But the remedy was obvious, and was applied as soon as ever it could be secured in sufficient amounts. Naturally it takes some little time to prepare, or otherwise, it needs several weeks to render horses highly immune, but once available, instructions were given that every wounded man should be inoculated against tetanus. Here are the results which I have abstracted from a paper by General Sir David Bruce, Chairman of the Committee appointed to investigate this matter of tetanus and its treatment. At first, I should explain, the amount of the serum at our disposal being by no means excessive, too small a number of units were injected into each man, and occasionally cases occurred in which, despite inoculation, the disease showed itself. Eventually an increased dose was made official, and from that time onwards tetanus was practically obliterated from among our troops.*

I should mention that the members of Sir David Bruce's Committee found another very possible cause of failure in a certain number of cases. They discovered that just as there are typhoid and paratyphoid organisms, so there exist in nature at least four distinct strains of the tetanus bacillus, so that the serum obtained from horses inoculated against one strain only of the bacillus would not be as effective in the cases of those infected with another strain as it is in those infected with the homologous strain.

Spotted Fever (*Cerebrospinal Meningitis*).—Epidemics of spotted fever have developed at irregular intervals in Europe and North America all through the last century, epidemics that have struck terror, so few of those attacked surviving. Mostly it is young children that have been the victims, but with this it is not a little remarkable that numerous barrack epidemics have been recorded. Since the century opened there have been serious epidemics in New York and the great cities of the United States, spreading into Canada. In the British Isles there was in 1906 to 1908 a similar epidemic, with heavy death roll in Belfast, spreading to Glasgow; another in Nottingham in 1910; while ever since then scattered cases and groups of cases have been recorded in Great Britain, until in 1912 cerebrospinal meningitis was made a compulsorily notifiable disease.

Then through the rain-swept winter of 1914–15 the disease made itself felt in the Expeditionary Force. There had been some dozen cases recorded among the civil population of England and Wales in the first three weeks of October 1914. The first military case was reported in the fourth week of that month. Here, as a Canadian, let me say that there is no evidence that the Canadian troops are responsible for the subsequent epidemic, as, again, absolutely none that they introduced a specially malignant form of the disease. It is true that cases had occurred at the Valcartier Camp, and three cases on ship-board when the first contingent crossed to England. The first case among the Canadians on Salisbury Plain was discovered upon October 18th, the first among British troops in the same week, but no evidence has been adduced to show any relationship between this first British military case and the Canadians. In all, between October 18th and the following May 1st, 50 cases occurred among 30,000 Canadians, with 36 deaths.

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* From the first No. 3 Canadian General Hospital employed double the official dose, with the result that not a single case occurred in the thousands of surgical cases treated there.

The war has taught us salutary lessons regarding the disease, notably that it follows overcrowding and bad ventilation. Reduce the number of men in a hut and see that the air is kept fresh, and the cases fall off rapidly. Much had been hoped from the employment of the Rockefeller anti-serum. This had been elaborated during the New York epidemic and had distinct good effects in America, as also in Belfast eight years previously; but the serum provided from America in 1914 and 1915 appeared to be absolutely without effect. Its failure led to a careful investigation by the Army authorities at Millbank, which took two main directions. In France attention had already been called to the existence of meningococci proper and parameningococci, and Major Ellis, of the Canadian Army, made a careful study of the bacteriology of these Army cases, thereby isolating three allied but distinct strains of meningococci. Simul-

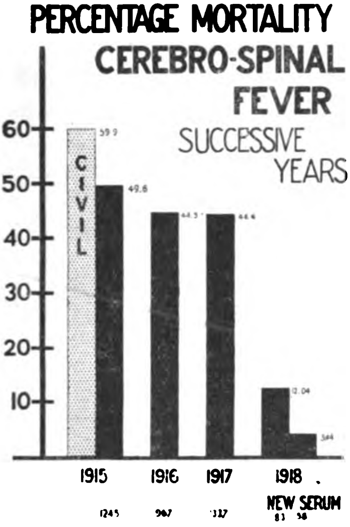
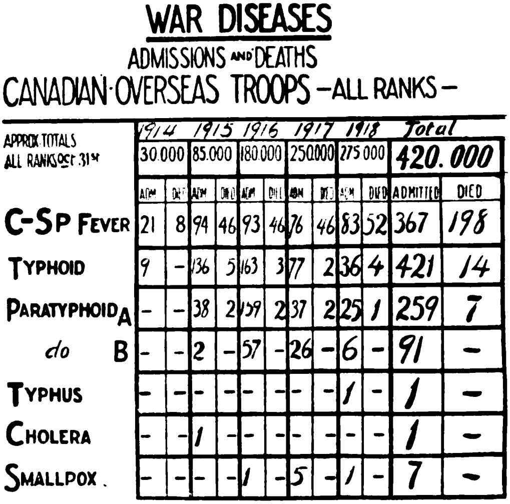


Fig. 3.

taneously Major, now Colonel, Gordon, at Millbank, isolated four, and these are now generally recognised. Following upon this Colonel Gordon and his colleagues have developed powerful anti-serums both mono-valent and poly-valent (protecting against all the strains), and with progressive improvements in these serums the mortality from the disease has been reduced to a very striking degree. As a matter of fact, Colonel Gordon and Major Hine have prepared a serum so potent that, whereas in the first two years of the War the mortality from this disease was 50 per cent and over, now in the last 83 unselected cases it has been reduced to 12 per cent; or, if we deduct the 15 cases in which experience has shown that the treatment was administered too late to be operative, to 3·44 per cent. By isolation of carriers, improved camp sanitation, and the employment of improved anti-toxins or anti-serums, what threatened to be a widespread epidemic has been prevented from spreading, and has been brought well under control.



WAR DISEASES

ADMISSIONS AND DEATHS

CANADIAN·OVERSEAS TROOPS —ALL RANKS—

	1914		1915		1916		1917		1918		Total	
APPROX. TOTALS ALL RANKS OCT. 31st	30.000		85.000		180.000		250.000		275.000		420.000	
	ADM	DIED	ADM	DIED	ADM	DIED	ADM	DIED	ADM	DIED	ADMITTED	DIED
C-SP FEVER	21	8	94	46	93	46	76	46	83	52	367	198
TYPHOID	9	–	136	5	163	3	77	2	36	4	421	14
PARATYPHOID A	–	–	38	2	159	2	37	2	25	1	259	7
do B	–	–	2	–	57	–	26	–	6	–	91	–
TYPHUS	–	–	–	–	–	–	–	–	1	–	1	–
CHOLERA	–	–	1	–	–	–	–	–	–	–	1	–
SMALLPOX.	–	–	–	–	1	–	5	–	1	–	7	–

FIG. 4.

So much for the outstanding triumphs, nor, to give you an impression of how great was our conquest over infectious disease—at least at the main seat of war in France and Flanders—do I think that I can do better than give you this analysis of the incidence of the main infections in the Canadian Expeditionary Force. It deserves note that vaccination against smallpox was compulsory, and that there was not a single death from this disease.

It would be wrong, however, to give you a sketch that was all high-lights; to give these their proper value the more sombre background of failures and partial failures must at least be indicated. There are other infections which, notwithstanding abundant and most conscientious laboratory investigations, we have been unable to arrest. Judging from certain letters in the daily press, the ordinary public does not realise the difficulties of bacteriological research, the patient way in which test after test has to be made, one man contributing this little advance, another man another, until the problem is narrowed down until we can predicate the nature of the causative organism, and can then take the proper steps to isolate and discover the means of cultivating that organism and determine its properties and habits, and can devise the proper means of arresting its growth and eradicating it. All this means, too frequently, months and sometimes years of unremitting labour. Influenza, for example, came upon us suddenly, and despite intense laboratory activity in all countries, the end of the War has come and we are still uncertain as to the causative organism. Tens of thousands of cases of bacillary dysentery occurred just along one front in the Gallipoli campaign. Indeed we may, with considerable justice, ascribe the failure of that campaign to the depletion in the ranks from dysentery. Malaria, which, as Sir James Barrett has recently pointed out, caused the failure of Mark Antony's Egyptian campaign, which came to the rescue of Saladin and the defeat of Richard Cœur de Lion and the Crusaders, and gave Napoleon his first defeat at Acre at the hands of a junior British officer, came near to rendering futile both the Salonika and Palestine campaigns.

Thanks to the brilliant work of Laveran in France, of Sir Ronald Ross in this country and in India, of Celli in Italy, and W. B. MacAllum in Canada and the States, we know the life-history of the malarial parasite, and are fully aware of the fact that destroy the anopheline mosquito—prevent it from breeding—and a district becomes no longer malarious. When war is conducted in regions where the campaign against the mosquitoes has not been carried out, then inevitably soldiers become infected. In the Salonika area, in Palestine, East Africa and Mesopotamia, our soldiers have fallen victims to malaria truly by the hundreds of thousands, and that just because we cannot wage wars in kid gloves, cannot tuck our soldiers into bed within mosquito curtains at sundown, or confine our operations to upland country away from the swamps where mosquitoes breed. Here is a case in which the end of the war has come and still despite all the research that has been carried out we seem little nearer adequate protection of our troops and triumphant prevention. Yet even here our knowledge of the natural history of the disease has materially helped us, and we were able to reduce the losses from it sufficiently to bring the East African campaign to a victorious conclusion. The most brilliant act of the whole war, Allenby's conquest of Palestine, depended for its success upon a knowledge of the natural history of malaria. Only by the most rapid advance could Allenby push his men through the malarial district of Palestine and overcome the Turk before they in their turn were overcome by malaria. But for this rapid action on his part this last crusade would have met with the fate of the earlier.

Nor did we work out the nature of trench fever until the war was almost over. The same is largely true regarding gas gangrene and its arrest. That we did not overcome the bane of all armies, venereal disease, was not, however, due to want of scientific knowledge, but to the hesitancy of those in authority and fear of putting in motion those procedures which would have kept these foul diseases under.

After all, the fight against the infections is very like that against the Hun. Our defences in one area have been so well placed and so well constructed as to keep the enemy at bay with ease; in another area, as around Ypres, our protection has been incomplete and the situation so unfavourable that we have only held on with enormous losses; at yet another point along the line a sudden attack for which we were not prepared may have led to rapid retirement and loss of ground, as on the Somme last spring. Of such a nature was the recent epidemic of influenza, which happily, like the German advance, wore itself out, but for a time led to a terrible mortality. The position regarding the venereal disease may be likened to the Gallipoli campaign—terribly costly, with unnecessary failure. A fuller knowledge of psychology and a fuller belief in the efficiency of preventive medicine would have converted failure into success. The methods of preventing these diseases and of reducing their ravages to inconsiderable proportions were there, only we were afraid to act. We thought the opposition would be too great.

WHAT LED TO SUCCESS.

Yet surveying our operations in general in our fight against disease, as against the Hun, we have come out well, and looking back I am inclined to think that three factors have especially contributed.

(1) It is to the credit of the late Director-General, Sir Alfred Keogh, that when he with his South African experience assumed office his first act was to place sanitary science in the forefront in the training of the army medical officer. He developed courses at Aldershot, not only for the officers of his own Corps, but also for regular officers at the Staff College, and for N.C.O.'s and men in the camp. Thus when the war came upon us the commanding officers of units and the sergeants and men of the little old Army had an intelligent knowledge of the principles of hygiene and preventive medicine; as a body they received willingly the recommendations of their medical officers, and they co-operated loyally. (2) Then, too,

in the Army Medical Corps itself medical research was appreciated. I have already mentioned members of that body who stand out among the foremost pathologists and bacteriologists of our generation: Sir Almroth Wright, Sir David Bruce, and Sir R. Leishman. And (3) a most potent influence during the war has been the whole-hearted encouragement and help of the Medical Research Committee, which under Mr. Lloyd George's Insurance Act was granted from National Insurance moneys a sum approximating £60,000 a year, which, with a wise patriotism, was with the war diverted to the advance of military medicine and research. Time forbids that I detail the many ways in which this Committee, with its energetic secretary, Sir Walter Fletcher, has placed its resources at the disposal of those, in and out of the army, wishful to work out the medical problems of the war.

SHELL-SHOCK.

And this, let me add, not only for prevention, but for treatment. Remembering my title, I cannot close without calling your attention to at least outstanding advance in medical treatment. I have already referred to the extraordinarily good results from the development of a national orthopædic surgery under the stimulating influence of Sir Robert Jones, whom we may without hesitation claim as the greatest military surgeon of this war—the Ambroise Paré of the twentieth century. A triumph equally great has been that over a condition so uncommon in previous wars as to be scarce noted, but one which in this war assumed for a time very serious proportions. I refer to the loss of self-control brought about by the intensity and gravity of the artillery warfare—the noise, the concussion, the frightfulness, the obvious impotency of the soldier, whether in the trenches or in the open, when exposed to shells which in size, explosive power and number exceeded anything even dreamt of in previous campaigns.

In the first two years of the war little could be accomplished to mitigate the profound breakdown, mental, sensory and motor, of the piteous sufferers from "shell-shock"—men rendered deaf, or dumb, or blind, or unable to walk from tremors and paralysis, and suffering from oft-repeated and terrifying dreams. Thanks to the studies of the neurologists, cases of malingering, of mere concussion, and of organic nerve diseases were distinguished from cases of shell-shock proper. At first these cases were sent home to England, and here, not properly cared for and treated, they went from bad to worse. Next, it was noted that these cases in the male were of much the same order as hysteria in the female, that they responded to suggestion of various orders and modes of application, and this the more readily the sooner after the onset of the condition of shock. And at the beginning of the third year of the war it was found, here following our allies, the French, that the best results were obtained, not by posting these cases to base hospitals and over the Channel, but

WEEKLY EVENING MEETING,

Friday, February 14, 1919.

The Right Hon. LORD WRENBURY, P.C. M.A., Vice-President, in the Chair.

PROFESSOR CARGILL G. KNOTT, D.Sc. LL.D., Gen. Sec. R.S.E.

The Propagation of Earthquake Waves through the Substance of the Earth.

[ABSTRACT.]

IT has long been recognised that every earthquake is the source of a succession of elastic waves propagated through the earth and over its surface. Three distinct types of waves have been discriminated the one from the other. Two of these consist of elastic vibrations transmitted along definite paths or rays and emerging at corresponding distances from the epicentre of the earthquake origin. It is now universally admitted that these two types of waves are the compressional and distortional waves of elastic theory, frequently known as the longitudinal and transverse waves. Of the many workers whose investigations helped to elucidate the character of these waves one calls for special mention—namely, the late Professor John Milne—who began his seismological studies while Professor of Mining in Japan in the last quarter of the nineteenth century, and continued them at Shide, Isle of Wight, to the day of his death in 1913. Supported by the Seismological Committee of the British Association, Milne was able to establish at a large number of stations all over the earth's surface a certain simple type of instrument for recording the tremors which, originating at the earthquake source, were sufficiently strong to affect the delicate seismometers installed at these stations. From a comparison of the records obtained at these stations and at other stations provided with different types of instruments, the following points were soon established :—

(1) A good earthquake record has an unmistakable character, the well-marked first movement being followed after a certain interval of time by the advent of a larger and well-marked second movement.

(2) The interval of time between the advents of the first and second characteristic movements increases with the distance of the station from the epicentre.

(3) From the measurement of this time interval on any seismogram the distance of the epicentre from the station at which the

record was obtained could be estimated with fair accuracy; and hence a comparison of the records obtained at different stations of the disturbances due to the same earthquake led to the determination of the position of the epicentre and of the approximate position of the earthquake origin.

With more accurate instruments of the Galitzin type, which may be regarded as the finished product evolved from the various types of instrument constructed by different investigators, it is possible under certain conditions to determine the position of the epicentre from the record of one set of seismometers at a given station. The steady accumulation of observations has led to a gradual improvement of our knowledge of the times of transmission of the two types of earthquake wave over arcs of different size. It is usual to call these types of wave the Primary wave and the Secondary wave, both being believed to be elastic waves through the body of the earth. If, as is probable, the Primary is the compressional wave, and the Secondary the distortional wave, their velocities of propagation are given by the expressions—

$$\sqrt{\frac{k + \frac{4}{3} n}{\rho}} \quad \text{and} \quad \sqrt{\frac{n}{\rho}}$$

where k is the incompressibility or resistance to change of volume, n the rigidity or resistance to change of form of the material of the earth, and ρ the density. Thus the Primary wave must obviously outrun the Secondary, since its speed of propagation is the greater. If the speed of propagation did not change from point to point within the earth the rays would be straight lines, just as the ordinary and extraordinary rays in a piece of Iceland spar or other doubly refracting crystal are straight within the crystal. In such a case the rays would be chords to the sphere, and it would be a simple matter to calculate the speeds of propagation. But a little calculation soon shows that the rays cannot be straight; and the question naturally arises, What is their form?

There are several mathematical ways of attacking this problem, which has exercised the attention of several investigators. The most elegant way is to treat it by Hamilton's general method applied to the problem of the brachistochrone, or path of shortest time from point to point—that is, with no assumption as to the manner in which the speed of the disturbance varies with depth below the earth's surface, find expressions for the form of the ray and for the time taken when the disturbance passes from one surface point to another. Now, the only data of observation we have are the times of transit over arcual distances up to about 110° or 120°. Beyond this distance the characteristic form of the record is no longer distinguishable, and no certain conclusions can be deduced from its appearance.

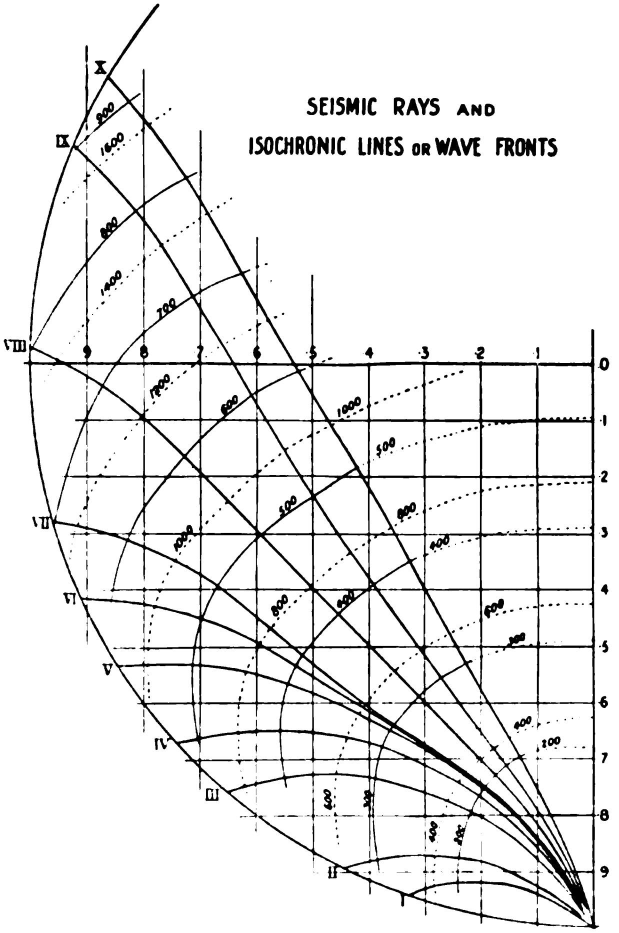
Unfortunately the expressions on which the value of the time and the form of the ray depend contain an unintegrable definite integral, involving the speed *v* as an unknown function of the distance from the centre of the earth. By assuming manageable forms for this function, various investigators have obtained empirical solutions sufficiently satisfactory to lead to conclusions not far from the truth. Such indirect and empirical methods are, however, not entirely satisfying. It is claimed that in the present discussion the problem has been worked out for the first time by an absolutely rigorous method from the data of observation, no assumptions being made as to the functional relationship connecting the speed of propagation and the distance from the earth's centre. The method is a numerical solution of an integral equation, giving each speed and the corresponding distance in figures. It will be found explained in detail in a paper read before the Royal Society of Edinburgh in November 1918 (see Proceedings R.S.E., vol. xxxix, pp. 157–208). It will suffice to indicate a few of the results obtained.

With the exception of the seismic ray which starts downwards towards the earth's centre and presumably emerges at the antipodes of the epicentre, all the seismic rays begin by curving so as to have the concavity towards the surface. This means that the speed of propagation of each type of wave increases as the depth below the surface increases. The outward concavity characterises the whole length of all rays which emerge at distances less than about 60° from the epicentre. But for greater arcual distances the ray straightens out at its middle and deeper part, and even becomes distinctly convex towards the surface. This change in the sign of curvature takes place at a depth equal to about three-tenths of the earth's radius. At this depth accordingly the speed of the wave which for shallower depths increases with the depth begins to diminish with increase of depth.

The forms of several of these seismic rays are shown in the accompanying figure, which represents fully a quadrant of a diametral section of the earth through the position of the epicentre. The sinuosity begins to appear in Ray VI. and is most marked in Ray VII, which emerges at an arcual distance of 73° from the epicentre. Ray VIII becomes practically straight throughout a considerable length, indicating that the speed is not changing with the depth.

The rays shown in the figure belong to the Primary wave. If the rays of the Secondary wave were placed on the same figure they would deviate very slightly from those of the Primary. In the figure the wave-fronts of the Secondary waves are entered in broken lines to distinguish them from the wave-fronts of the Primary waves which are drawn in full. The numbers attached are the times in minutes for the wave-fronts to pass from the earthquake origin to the marked position. The broad similarity between the two sets of wave-fronts shows that the differences between the forms of the rays of the two types of wave are comparatively slight.

In addition to the different velocities which characterise the two types of wave there is another point of difference which seems


SEISMIC RAYS AND
ISOCHRONIC LINES OR WAVE FRONTS
X
IX
VIII
VII
VI
V
IV
III
II
I
900
1600
800
1400
700
1200
600
1000
500
800
400
300
200
9
8
7
6
5
4
3
2
1
0
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8
9

worthy of attention. When the speeds of propagation are plotted against the corresponding distances from the earth's centre, the curves representing the two types of wave are similar in form, rising rapidly to a maximum and then becoming practically parallel to the line of distances. The distances at which the speed ceases to increase and becomes constant for shorter distances from the earth's centre is however different in the two cases. This is brought out by the following approximate representation of the values of the speed v in terms of the distance r from the earth's centre, the units being the kilometre and second. (The earth's radius is 6378 kilometres.)

THE PRIMARY WAVE.

From $r = 6250$ to $r = 5710$, $v = 32{\cdot}78 - 0{\cdot}0040\,r$
$r = 5620$ $r = 4790$, $v = 28{\cdot}27 - 0{\cdot}0032\,r$
$r = 4800$ $r = 3500$, $v = 12{\cdot}8$

THE SECONDARY WAVE.

From $r = 6350$ to $r = 5700$, $v = 17{\cdot}37 - 0{\cdot}00208\,r$
$r = 5620$ $r = 5040$, $v = 15{\cdot}93 - 0{\cdot}00181\,r$
$r = 5000$ $r = 3000$, $v = 6{\cdot}85$

Thus the speed of propagation reaches its maximum at a less depth for the Secondary wave than for the Primary wave, less by about 200 kilometres, or one-thirtieth of the earth's radius.

This fact suggests a physical change in the material of the earth which first affects the rigidity. As already mentioned no good records of the advent of the Secondary wave are obtained at arcual distances greater than about 110° from the epicentre. The depth reached by a ray which emerges at this distance is not much more than half the earth's radius. It thus appears that a ray of the Secondary type of wave—that is, a ray of the distortional wave—cannot penetrate deeper than about six-tenths of the earth's radius. The material of the earth may be supposed to undergo a gradual change from distance 0·5 of the earth's radius as measured from the centre to distance 0·4. At position 0·5 the material behaves like an elastic solid with the two elastic moduli of wave velocity $k + 4\,n/3$ and n; but when the position 0·4 is reached the material has lost its rigidity and has become what might be compared to a highly compressed substance with incompressibility k and rigidity zero. Under these conditions the distortional wave will be gradually stifled and ultimately cease to exist. Its energy may pass into the compressional form or it may simply be transformed into heat under the influence of viscosity to shearing motion.

SUMMARY.

For the first time, by a rigorous mathematical method, the forms of the seismic rays have been deduced directly from the data of observation. The seismic rays are on the whole concave outwards, indicating that the speeds of propagation increase with depth below the earth's surface until a depth equal to about three-tenths of the earth's radius is reached. At this depth the speeds of propagation tend to, or reach a constant value, and then fall off slightly for greater depths, certain rays showing a convexity outwards. The data of observation are insufficient to enable us to trace distortional waves which reach a depth lower than six-tenths of the earth's radius. The evidence is that at or near this depth the distortional wave is killed out, so that over arcual distances from the epicentre greater than 120° there is no characteristic appearance of the Secondary or distortional wave in the seismograms. The hypothesis suggested by these facts and deductions is that the earth consists of an elastic solid shell down to a depth of about half the earth's radius, that at this depth the rigidity begins to break down, and that finally, at a depth of six-tenths of the earth's radius the elastic solid shell gives place to a non-rigid nucleus of measurable compressibility. This hypothesis is broadly similar to the views advanced by R. D. Oldham in 1906.

[C. G. K.]

WEEKLY EVENING MEETING,

Friday, February 21, 1919.

General E. H. Hills, C.M.G. D.Sc. F.R.S., Secretary,
in the Chair.

A. T. Hare, M.A.

Clock Escapements.

The most ancient instruments for measuring time were probably some kind of sundial. Something of the kind is, no doubt, referred to in 2 Kings xx. and Isaiah xxxviii., where it is stated that the shadow moved back ten steps on the steps of Ahaz (for that is the literal translation). Herodotus ("Euterpe," cix.) tells us that the Babylonians introduced to the Greeks the πόλος and the γνώμων, no doubt some forms of sun-instruments. Frequent allusions are found in the classics to the clepsydra, which was made in various forms, always depending, however, upon the approximately uniform flow of water through a small hole.

But clocks, properly so called, cannot be traced with certainty earlier than the fourteenth century. In 1348 a curious iron clock was sent over from Switzerland, and was until recently kept in Dover Castle. It is now in the Science Museum at South Kensington. It is interesting as having no pendulum or balance-spring (both much later inventions), but, instead, a vertical spindle carrying a horizontal traverse loaded at the ends with weights. This vertical spindle has two pallets projecting from its sides, approximately at right angles to each other, which engage alternately the uppermost and lowermost tooth of a contrate wheel the axis of which is horizontal and in the same plane with the vertical axis first referred to. This is the "verge" escapement, which was for long afterwards used in both clocks and watches. No good timekeeping was possible with such an arrangement. Gravity did not come into the problem, and the speed of the movement was only restrained by its energy having alternately to create and destroy angular momentum in the swinging arms. The force of the train, however variable, was paramount.

The next step in horology, and undoubtedly the most important which has ever been made, was the application of the pendulum to clocks by the Dutch physicist and astronomer, Christian Huygens, in 1657. Galileo had discovered, about sixty years earlier, the isochronism (since found to be only approximate) of a swinging body, but, in spite of efforts made after his death to claim priority for him in the invention of the pendulum clock, the evidence has not convinced historians of his title to that honour.

Huygens, being aware of the fact that the motion of a particle under gravity was only isochronous, independently of the extent of the arc of swing, when the body describes a cycloid, and knowing the property of that curve to reproduce itself as an involute of an equal cycloid, attempted to secure the desired isochronism by suspending his pendulum from a silk thread which swung between two cheeks of brass cut to the shape of the cycloid, thus obliging the bob to trace an involute. But the silk was so affected by the weather that no good result ensued.

Another objection to the verge escapement was the large arc of swing necessary to permit the escapement to unlock itself. Huygens attempted to overcome this difficulty by making the verge the axis, not of the pendulum-crutch, but of a pinion gearing into a larger wheel to the arbor of which the crutch was attached. This construction permitted the angle of swing to be reduced at pleasure, but more friction was introduced, and little improvement was effected.

The calculation of the time of swing of a free pendulum describing a circular arc can only be made approximately, but the approximation can be carried as far as desired, and as the arc of swing is never large, a few terms suffice. This is the formula :—

$$T = \frac{\pi k}{2\sqrt{gh}}\left(1 + \frac{1}{4}\sin^2\frac{a}{2} + \frac{9}{64}\sin^4\frac{a}{2} + \ldots\right)$$

from which, by differentiation,

$$\frac{dT}{da} = \frac{\pi k \sin a}{16\sqrt{gh}}\left(1 + 18\sin^2\frac{a}{2} + \ldots\right).$$

Here T is the time of swing of the pendulum from its highest position to the vertical, and a is the semi-angle—that is, the angle turned through from the highest to the lowest position. Now of the factors making up the expressions on the right-hand side of these equations, only π and g and the numerical coefficients can really be considered as constant. It has been suggested that even g may one day be shown to be variable. As for h and k—that is, the distance from the axis of motion to the centre of gravity and the radius of gyration respectively—these are well known to be dependent on temperature, and an interesting account might be given, if time permitted, of the evolution of the compensated pendulum. The recent discovery of alloys of iron and nickel, the coefficient of expansion of which is very low, has much facilitated this.

The factor which has most influence on the value of T is a, the angle of swing. The formulæ show us two things : first, that the wider the arc of swing the more a clock will lose, and, secondly, that a given small variation of arc is less harmful when the whole arc is small than when it is great. There are practical reasons, however, for not making it *too* small, which have led to the adoption of arcs of two or three degrees on each side of the vertical as, on the whole, the best.

This table gives the losing rate for variations of arc :—

Semi arc ° ′	Daily loss s.	Difference
0	0	
0 15	0·1	0·1
0 30	0·41	0·31
1	1·65	1·24
1 30	3·70	2·05
2	6·58	2·88
2 30	10·28	3·70
3	14·31	4·53
3 30	20·16	5·35
4	26·33	6·17
4 30	33·32	6·99
5	41·14	7·82

It must be remembered that these figures only relate to a *free* pendulum, and with some escapements the errors introduced mask this result completely.

Many attempts, some of great ingenuity, were made to get better results from the verge, especially as regarded the reduction of the arc, but they were all superseded by the anchor, or recoil, escapement, invented (most probably) by the celebrated Dr. Hooke, and first made by William Clement in 1675. This is the escapement still used in all common clocks, but it has disadvantages which render it unsuitable for high-class work. The train exercises great "dominion," as it used to be called, over the pendulum, and is assisting gravity the whole time, hindering the rise of the pendulum and accelerating its fall, so that T may be considerably diminished when the train has been recently oiled, without any corresponding variation of *a*.

But in 1715 George Graham, pupil of Tompion (both of whom were so esteemed as to be accorded burial in Westminster Abbey), made a most important modification of the anchor. He removed most of the flukes, leaving only a small sloping part near the tip, by sliding along which the extremity of the scape-wheel teeth could give the necessary impulse to the pendulum. The rest of the fluke he fashioned so that it should be a portion of a circle having its centre on the axis of the crutch-arbor, thus entirely preventing recoil of the movement, and, to a great extent, releasing the pendulum from the "dominion" of the train. During the time when the circular part of the fluke is passing along the tooth of the scape-wheel the motion of the train is entirely held up, and it is neither doing work on the pendulum nor having work done on it. The device is consequently known as the "dead-beat."

Numbers of escapements were devised after Graham's invention, which, though differing much from it in design, were, nevertheless, broadly speaking, mechanical equivalents of it. Such were Thiout's, Vérité's, Perron's, Leonhard's, Vulliamy's, Robert's, Berthoud's, Lepauté's, and Brocot's.

The designer of a turret-clock, however, always has in mind the serious variations in the force of the train caused by wind or snow on the hands, as well as by the thickening and drying of the oil on the bearings and the cutting and wearing of the pivots and of the teeth of the wheels and pinions. It was, therefore, long ago recognised that the proper function of the clock-train was not to drive the pendulum, but to record the number of its swings—that is, to tell the time—and to keep wound a smaller clock which should be independent of these disturbances, and could be made very simple, and even reduced to one wheel, if often enough rewound. This construction was proposed by Huygens, who did so much for the science of accurate time-keeping. The principle of these "remontoirs," as they are called, is very much the same in all. Some rewind a little weight, others keep a spring wound, but in every case, directly or indirectly, the pendulum has to unlock the rewinding mechanism by means of some device which is itself an escapement, and this cannot be effected without some friction.

From the train-remontoir it is an easy step to the next great improvement. The question naturally arises: "Why rewind the train in the middle? Why not simply relift the pallets and let them fall by gravity on the pendulum?" This question was answered about the year 1716, when Alexander Cumming produced the first of the series of gravity escapements which have done so much to make the accurate turret-clock a possibility. His escapement is rather complicated and has several points where there is friction, and very soon after it was greatly simplified and improved by Thomas Mudge, a pupil of Graham's. Fig. 1 shows Mudge's escapement, and will be easily understood.

The tooth marked 1 has just lifted the gravity piece A'B', and is resting on the dead face. The pendulum, moving to the right, is just about to lift the gravity piece, causing the dead face to slide along the tooth until it is clear of it. The wheel is then free to turn further, and the tooth marked 2 lifts the other gravity piece AB in a similar way. When the pendulum has attained its maximum elongation to the right (carrying A'B' with it) and begins to return, the pallet on A'B' falls midway between teeth 1 and 3, thus falling rather farther than it rose, the balance of work done on the pendulum serving to maintain the latter in motion against the resistances. Each gravity piece is lifted by the wheel at a time when the pendulum is out of contact with it, and so the action of the train cannot disturb the pendulum except by the friction of the dead faces.

There is, however, a source of error to which Mudge's escapement is liable which was sufficient to condemn it. The driving power had to be ample, and there was danger either that the gravity pieces might be thrown clean off the wheel, allowing the latter to race and destroying all timekeeping, or that, if this complete "tripping" did not occur, they might, at all events, be thrown a little too high, so that the teeth of the scape-wheel, instead of resting in the exact corner, as tooth 1 is seen to be doing, would rest on the dead face nearer its extremity, and probably hold up the gravity piece, by friction, higher than it should. This fault was called by Lord Grimthorpe "approximate tripping," and if it occurred the constancy of the maintenance would be lost. This might probably have been cured by the use of a dashpot, with which Mudge's escapement would have been very considerably improved.

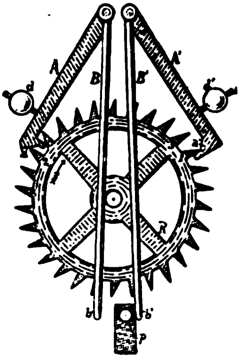

Fig. 1.

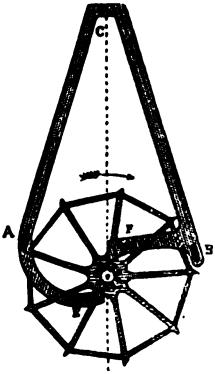

Fig. 2.

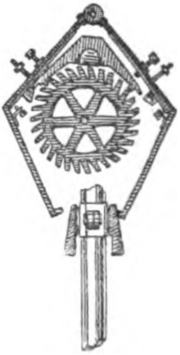

Fig. 3.

Mudge's escapement was followed by Bloxam's, the action of which will be obvious from Fig. 2. It is still to be seen in action in Bloxam's own clock, which is now, by his nephew's permission, at the Science Museum. The noteworthy feature in it is that the locking arms are much longer than the lifting teeth, so that the friction of unlocking is much reduced.

It was on Bloxam's design that Lord Grimthorpe improved in the construction of his well-known "double three-legged gravity escapement," used for the first time in the great clock in the Houses of Parliament. The principal feature in this escapement is the long wind-fly, which moderates the shock of impact of the teeth on the pallets, and which the large angular movement of the scape-wheel (60° at each tick as against 20° in Bloxam's) rendered effective.

A new principle was introduced into the gravity escapement by Capt. Kater about the year 1840, and is described in vol. cxxx. of the Phil. Trans. Fig. 3 is taken from Kater's paper, and shows clearly the design. The gravity pieces are lifted alternately as in Mudge's and Bloxam's constructions, but they do not themselves unlock the escapement, merely serving to upset the equilibrium of a heavy piece (seen in the figure above the wheel), which does the unlocking, but, owing to its high moment of inertia, gets slowly under way and so unlocks the wheel only when the gravity piece then in contact with the pendulum is no longer touching it.

Vérité produced a gravity escapement in which pivot friction was got rid of, but this escapement had four little balls hanging from four silk threads, and was somewhat delicate and complicated.

It occurred to me some time ago that Kater's principle might be applied in such a way that the pendulum should be entirely freed from all friction whatever, while the impulses given to the pendulum were exactly uniform. A full description of this escapement will be found in Patent Office Specification No. 113,501, but it may be said, very briefly, to consist in two little weights which rest alternately on the two ends of a rocking frame having considerable moment of inertia, and on two little upright stems at the ends of arms fastened to the pendulum near its point of support. When the rocker is horizontal, and the pendulum at rest and vertical, things are so adjusted that the weights are resting indifferently on both the pendulum arms and the ends of the rocker. If, then, the pendulum is pushed to one side, say the right, it carries the right-hand little weight upwards, relieving the rocker of its weight, and deposits on the opposite end of the rocker the other little weight. This upsets the equilibrium of the rocker, which commences to turn over, and so releases the scape-wheel, which turns the rocker back rather beyond the horizontal in the sense opposite to that of its last motion, so that when on its return the pendulum again exchanges weights with the rocker, it deposits the right-hand weight at a lower level than that at which it was picked up. The escapement is simple, and a clock fitted with it has given results which are encouraging.

Before concluding, I must refer to a remarkable series of papers which commenced last year to appear in the Proceedings of the Royal Society of Edinburgh by Prof. R. A. Sampson, the Astronomer Royal for Scotland. Prof. Sampson is, as all astronomers must be, much interested in accurate timekeeping, and has experimented with three different clocks, having escapements which I must very briefly describe. One is by Mr. Cottingham, and is essentially the same as an escapement which the late Sir David Gill, then Astronomer Royal at the Cape, had imagined. The pendulum is driven by a gravity piece which, so long as it is in contact with the pendulum, by that very contact completes an electric circuit which holds up an armature against the poles of an electromagnet. This armature is itself the stop which limits the travel of the gravity piece. The latter, therefore, goes on impelling the pendulum until it is brought up against the armature. When this happens the gravity piece is left behind by the pendulum and the circuit is broken. At once the armature falls against a stop, and the gravity piece is lifted, so that the pendulum takes it up again at a higher level than that at which they parted company. Sir David Gill found trouble from the slight adhesion which exists between two metallic surfaces when a current is broken between them, and gave much attention to experiments designed to avoid this. I do not know how far he succeeded, but it seems clear from Prof. Sampson's paper that the escapement is very successful now. The idea has probably occurred to many people. I began making a clock about thirty years ago on what was practically the same principle, but gave it up because at that time it did not seem practicable to find a battery capable of giving a current lasting nearly half a second for each second that passes.

Another of Prof. Sampson's clocks is driven by an escapement invented by Riefler, of Munich, which is unlike any of those we have been considering, and in which the necessary energy is communicated to the pendulum by bending the suspension spring. The block from which the suspension spring hangs, instead of being fixed as immovably as possible, which it generally is, is supported on knife-edges, and the suspension spring, which, of course, always tries to keep straight, causes the block to turn on these edges, and so unlock the scape-wheel, which bends the spring back against the motion of the pendulum and thus keeps it going.

The third escapement, which is being observed at Edinburgh, and the last I propose to refer to, is that adopted by the Synchronome Co., and belongs to the class where the action takes place at the bottom of the pendulum or of the crutch instead of the top. This is fully described in the specification of a patent granted to Mr. Shortt, and numbered 9527 of 1915.

So much for escapements.

We may, in conclusion, for a moment review the difficulties attending the accurate measurement of time and note how they have been attacked.

If ever a perfect clock is constructed it will certainly be a pendulum clock, and it will have to fulfil two conditions, necessary and sufficient. They are these :—First, the moment of inertia of the pendulum must be invariable ; and, secondly, the forces which act on it must be invariable. If these two conditions could be fulfilled, the last word in horology would have been said. So far, of course, neither condition has been fulfilled, but surprisingly good work has been done. As for the first condition, that the moment of inertia must be invariable, the chief difficulty is to avoid change by change of temperature. There are two ways of diminishing this change. The pendulum must be compensated in one of the well-known ways

—by Harrison's gridiron construction; or that of Graham by the expansion of mercury in the bob; or, again, by the zinc and iron combination used in many turret-clocks; or, best of all, by availing ourselves of the low expansion nickel-steel recently introduced by Guillaume. Also, for added security, the whole clock must be enclosed in a thermostatic chamber, as is done by Prof. Sampson at the Royal Observatory at Edinburgh. The other condition is much more difficult. There is, besides the almost inevitable friction of the escapement, the effect of the buoyancy of the air. This last can be avoided by enclosing the whole clock in a glass case, tightly fitted, in which the air can be slightly rarefied and maintained at a constant pressure below that of the atmosphere. This would seem to offer a very satisfactory solution of the difficulty. Temperature error and buoyancy error having thus been to a great extent mastered, we come back to the forces connected with the maintenance and recording of the motion as the principal sources of uncertainty. And let no one suppose that little has been effected. Perfection in this, as in other human pursuits, is doubtless unattainable, but we approach it asymptotically, and we are farther along the asymptote than might be imagined. Prof. Sampson tells us that in his thermostatic chamber and barostatic cases, and with the Riefler, Cottingham, and Synchronome escapements which he is studying, the errors average no more than one-hundredth of a second per day—that is, at the rate of one minute in sixteen years, if the clock could run so long without stopping—truly an almost miraculous accuracy, unrivalled, I imagine, in any physical measurement. Anyone, therefore, who hopes to improve upon this has a difficult task before him. If it is true that *le mieux est l'ennemi du bien*, it must be acknowledged that *le mieux* has against him a most formidable antagonist.

[The lecture was illustrated by a number of working models.]

[A. T. H.]

WEEKLY EVENING MEETING,

Friday, February 28, 1919.

THE RIGHT HON. LORD WRENBURY, P.C. M.A.,
Vice-President, in the Chair.

SIR OLIVER J. LODGE, D.Sc. LL.D. F.R.S. M.R.I.

Ether and Matter: Being Remarks on Inertia, and on Radiation, and on the possible Structure of Atoms.

[AMPLIFIED FROM THE LECTURE.]

PART I.—INERTIA.

WE are each of us flying through space at nineteen miles a second, probably much more. Nothing is propelling us; we continue to move by our own inertia, simply because there is nothing to stop us. Motion is a fundamental property of matter. No piece of matter is at rest in the ether, the chances are infinite against any piece having the particular velocity zero; every bit is moving steadily at some given speed, unless acted on by unbalanced force. Then it is accelerated—changed either in speed or direction, or both.

As a matter of fact we, like other bodies on the earth, are acted on by two slight unbalanced forces—one which makes us revolve round the earth once a day, like a satellite, the other which makes us revolve round the sun once a year, like a planet or asteroid. Our annual revolution is not because we are attached to the earth; we are not attached, but revolve as independent bodies, and would revolve in just the same time and way if the earth were suddenly obliterated: only then we should find the diurnal revolution transmuted into a twenty-four hour rotation round our own centres of gravity, and the excentricity of our annual orbit very slightly changed. In any case there is no propelling force, only a residual radial force producing curvature of path.

A railway train, or a ship moving steadily, is likewise subject to no resultant force. Propulsion and resistance balance. The whole power of an engine, after the start, is spent in overcoming friction. The motion continues solely by inertia. Any steadily moving body is an example of the first law of motion. You need not try to think of a body under no force at all; you cannot think of such a body on the earth, but you can think of one under no resultant force, i.e. under balanced forces. Such a body moves by reason of its inertia alone. It is in equilibrium: it is not at rest.

But we have no sense of straightforward locomotion, and not the slightest clue to either the magnitude or direction of our motion through space. We can ascertain approximately how the sun is moving with reference to our system or cosmos of stars, but we do not know at what rate that system is itself moving. For all we know it may be moving very fast: hundreds of miles per second.

We have a sense of acceleration however; we experience it in a lift as it begins to descend; and if the sensation is repeated often enough, as on a rough sea, the result is unpleasant. We have also a sense of rotation; we can tell when our vehicle—say a Tube train—turns a corner in the dark. Most animals appear to have a sense of rotation, apparently located in the ear. But we have no sense of direct translation; and we have so far failed to devise any instrumental means for detecting our motion through the ether of space.

The failure is not for lack of trying. Many experiments have been tried, but there is always some compensating effect; so we get no answer to the question—at what rate and in what direction are we moving? The best known experiment is that of Michelson and Morley, the result of which seems to assert that the ether clings to the earth, or that the earth is not moving through any kind of substance. But Fitzeau's classical experiment showed that a transparent body carried with it none of the internal ether of space; and experiments made by myself * at Liverpool in the nineties of last century show that a rapidly moving opaque body carries no external ether with it, that there is no perceptible viscous drag or cling between matter and ether, and accordingly demonstrates that stagnation or absence of relative ether drift past the earth is not a reasonable explanation of Michelson's negative result.

The two experiments together, in fact, ought to be taken as establishing the reality of the most interesting of all the compensating effects yet discovered, viz. the FitzGerald-Lorentz contraction of all matter in motion, which the electrical theory of cohesion renders so extremely probable. It only amounts to a 3-inch shrinkage in the whole diameter of the earth in the direction of motion; but it is enough. This slight contraction or change of shape in moving bodies I regard as the definite and interesting compensating effect in this case. Incidentally, moreover, it establishes the electrical, i.e. the chemical, nature of cohesion. For given that cohesion is a residual chemical affinity—due to the outstanding attraction of molecules composed of neutral groups of equal opposite electric charges, brought so near together that the attraction between molecules is no longer averaged to zero †—then, on orthodox Maxwellian electric theory, a

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* See Phil. Trans., vol. clxxxiv. (1913), pp. 727–804, and vol. clxxxix. (1897), pp. 149–166.

† See for instance my book on Electrons, chap. xvi.

diminution of this force due to lateral motion is inevitable. And the resulting lateral expansion or longitudinal contraction, or both, is of the right order of magnitude. So this acts as a previously quite unsuspected compensating effect, which exactly neutralises the drift effect otherwise to be anticipated. Thus, by superposition of two positive consequences of drift, the Michelson experiment, like every other yet made, declines to indicate that there is any drift at all.

Hence, after many such negative results, it seems to become hopeless to enquire experimentally as to our motion through ether. Unless indeed gravitation were exempt from the otherwise universal compensation. In that case the electrical theory of matter applied to the motion of planets might yield a residual result. But my recent enquiry into this problem has suggested that gravitation too is in the conspiracy,[*] and in that case there is some ground for the contention of the extreme Relativists, not only that we do not know our motion—with which everyone agrees—but that we never shall know it: and, in fact, that motion of matter through ether is a phrase without meaning.

I hope we shall not too readily shut the door on further attempts in this direction; and as a conservative physicist I may be allowed to lament the extraordinary complexity introduced into physics and into natural philosophy by the principle of relativity, as so remarkably and powerfully developed by the mathematical genius of Einstein, with complication even of our fundamental ideas of space and time. The complications do not commend themselves to all of us, and I for one should be glad to return to the pristine simplicity of Newtonian dynamics, modified of course by the electrical theory of matter; admitting the FitzGerald-Lorentz contraction, and admitting also the variation of effective inertia with speed. These things do not destroy, but supplement, Newtonian dynamics. They generalise it in a legitimate and intelligible manner. Such complications as these are clearly in accordance with truth and are to be welcomed; but the complicated theory of gravitation created this century by Einstein. and developed by his successors, and the consequent overhauling of space and time relations, do not at present commend themselves to me, nor I think to others of what I suppose must be called the older school.

Meanwhile the full-blown theory has the courage of its conviction and has predicted a definite result, viz. the deflexion of a ray of light by the sun's limb, equal to 1·75 seconds of arc. The prediction is going to be tested during the solar eclipse of May 29 this year, between Brazil and the Gulf of Guinea. Let the issue be clearly understood. If a star-ray grazing the sun is deflected ¾ second it will mean only that light has weight, that the wave-front not only simulates the properties of matter by carrying momentum —as we know it does from the investigations of Nichols and Hull, Poynting and Barlow, and others—but that it is even subject to gravity. For this would be the angle between the asymptotes of a cometary orbit when the comet is moving with the speed of light and passing close to the sun.* But the principle of relativity—through the refractive or converging influence of a strong divergent gravitational field—demands a greater deflexion than this, more than twice as great. So there are three alternate deflexions before us, to be settled by observation :—

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[*] See The Philosophical Magazine for August, 1917, and February, 1918, pp. 145, 155 and 156.

1·75 sec. ; 0·75 sec. ; and zero.

Let us hope that the result of this or of some other eclipse-opportunity may be definite enough to discriminate clearly and quantitatively between these three alternative values ; any one of which should be equally welcome to any lover of truth.

If the first answer is given decisively, it will be a conspicuous triumph for the theory of relativity, and will for a time be hailed as a death-blow to the ether. I claim beforehand that such a contention is illegitimate, that the reality of the ether of space depends on other things, and that the establishment of the principle of relativity leaves it as real as before ; though truly it becomes even less accessible, less amenable to experiment, than we might have hoped. Nevertheless the ether is needed for any clear conception of potential energy, for any explanation of elasticity, for any physical idea of the forces which unite and hold together the discrete particles of matter whether by gravitation or cohesion or electric or magnetic attraction, as well as for any reasonable understanding of what is meant by the velocity of light. Let us try to realise the position beforehand ; for we shall be handicapped in the progress of our knowledge of the relation between matter and ether until these fundamental things are settled, and until everyone agrees that the ether has a real existence. I want people generally to admit that the ether is itself stationary as regards locomotion, and that it is the seat of all potential energy ; and further, at least as a surmise, that it is the medium out of which matter is probably made, and in which matter is perpetually moving by reason of its fundamental property called inertia—a property the full explanation of which must, I expect, ultimately be relegated to and considered as a property derived from the ether itself.

I call this lecture *Ether and Matter*, but I might equally well have called it *Inertia*, for that is the main theme with which I have to deal—at least in this first part.

Is there anything else, besides matter, which possesses or seems to possess inertia? Faraday discovered that an electric current had a property which bore some analogy to inertia, a property clearly depending on its magnetic field. Every current, even a convection current, is necessarily surrounded by lines of magnetic force, and when the magnetic field is intense the current behaves as if it had considerable inertia. Faraday at first called the effect "the extra current." Maxwell called it "self-induction." The latter is the better name.

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* See, for instance, my paper in the Philosophical Magazine for August, 1917, page 93.

To show it I start a current in a circuit containing a stout ring of laterally subdivided iron round which the current-conveying wire is wound, and I put in circuit an instrument which only responds when the current has risen to nearly its full strength. A current usually rises, what is called, instantaneously, but here there is a very noticeable delay between pressing down the key and the response of the instrument. [Experiment.] The lag shown is only a second or two, but with care I can adjust it till it is a quarter of a minute. Such delay or lag in establishing a current would be fatal to electric telegraphy. In practice the delay is reduced to a minimum, by using its early values, and the actual response is exceedingly quick. Still, the law of rise of current is quite definite, there is no exception, it is only a question of degree; and the law is the same as that appropriate to the pulling of a barge on a canal. A barge gets up speed slowly, at a rate depending on its mass or inertia, and it ultimately attains a steady speed when the resistance balances the pull.

That is exactly the case of a steady current obeying Ohm's law, the E.M.F. is balanced by the resistance, the propelling force is zero, and the current flows by what we may call its own inertia—its own momentum.

To stop the current you must either increase the resistance or suspend the propelling force. If you interpose an obstacle suddenly, the motion stops with violence—a collision in the case of a train or barge, a flash in the case of electric current. This is what Faraday called "the extra current at break," and if you are holding the wires in your hand when a current is suddenly broken in a circuit of large self-induction you may get a nasty shock.

If you could abolish electric resistance a current would go on for ever without propelling force.

An amazing experiment has been made by Kamerlingh Onnes at Leiden, who first cooled a metal ring down to within four degrees of absolute zero by means of liquid helium, and then started a current through it by a momentary magnetic impulse. Instead of stopping in a minute fraction of a second, as usual, the current went on and on, not for seconds but for days. In four days it had fallen to half strength, and there were traces of it a week later. A most suggestive experiment as to the nature of metallic conduction, as well as a demonstration of the fly-wheel-like momentum of an electric current!

This electromagnetic analogue to mechanical momentum or inertia is explicable (or supposed to be explicable) in terms of the magnetic field surrounding the current, i.e. really (as I think) in terms of a property of the ether of space. It exactly simulates inertia; but is it an imitation or is it the same thing? Can it be said that an electric charge possesses inertia in its own right, and retains it always, as matter does, whether it be moving or whether it be stationary?

The question was brilliantly answered by your Professor of Natural Philosophy, Sir J. J. Thomson, so long ago as 1881. He calculated the inertia or quasi "mass" of an electric charge e, on a sphere of radius a, and showed that it was $m = \frac{2\mu e^2}{3a}$.

The μ need not be attended to now, though it is really the most important of all—being a great etherial constant of utterly unknown value*—but for our present purpose the μ merely signifies that the e must be measured in electromagnetic not electrostatic measure, when the formula is interpreted numerically with $\mu = 1$.

At the date 1881 this expression for true electric inertia, though an interesting result, seemed too absurdly small to have any practical significance. Take a sphere like a football, 20 centimetres or 8 inches in diameter; charge it till it is ready to give more than an inch spark, say up to 60,000 volts; then calculate the inertia or equivalent mass corresponding to the charge. If I have done the arithmetic right it comes out one-third of a millionth of a millionth of a milligramme (3×10^{-16}). Absurdly small! Yes, but not zero. And whenever a quantity is not nothing, there is no telling what importance may not have to be attached to it sooner or later. Nothing real can be so small as to be really negligible in the long run as knowledge progresses. Something at present unforeseen may bring it into prominence. So it has turned out in this case. The infinitesimal result of nearly forty years ago to-day dominates the horizon. It was in some sort the dawn of a new era in physics.

Consider it further. Clearly the inertia depends not on the charge only, but on its concentration. The radius of the sphere occurs in the denominator of the expression. The same charge on a sphere 2 centimetres in diameter would have ten times the inertia; on a sphere as small as an atom the inertia would be a hundred million times bigger still. But then even that is small; moreover an atom could scarcely be expected to hold such a charge. Nevertheless, allowing only a reasonable potential, it might seem that atomic inertia could be sensibly increased by an electric charge. But no, even on a sphere as small as an atom the concentration turns out insufficient; the effect is still excessively minute. Yet as electric inertia at given potential depends on linear dimensions, while material inertia depends on those dimensions cubed, there must be a size when the two are equal, i.e. when one might account for the other.

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* I have guessed that it is a density of 10^{12} grammes per c.c. $\div 4\pi$. See The Ether of Space, Appendix 2; also the Phil. Mag. for April, 1907.

Write the charge in terms of electrostatic potential V

$$e = \mathrm{K} a \mathrm{V}$$

then
$$m = \frac{2 \mathrm{K} a \mathrm{V}^2}{3 c^2}$$

where c is $1/\sqrt{(\mu \mathrm{K})}$, the velocity of light.

Put this expression for m equal to $\frac{4}{3} \pi a^3 \rho$, the ordinary mass.

Then the potential at which the two will be equal is

$$\mathrm{V}_1 = a c \sqrt{\left(\frac{2 \pi \rho}{\mathrm{K}}\right)}$$

which, for density of water and for a sphere 10^{-13} centimetres radius, is two volts; quite a reasonable electrolytic value, such as is to be expected among atoms.*

The moral of this elementary but not very satisfactory argument is that not for bodies of atomic size, but for something 100,000 times smaller in linear dimensions, is it possible to explain inertia electromagnetically. But, 40 or even 20 years ago, one would have said—there are no bodies of this size; nothing can be smaller than an atom! The strange thing is that, as nearly everyone knows now, bodies of this size have been discovered. They were isolated by J. J. Thomson in 1899, having been gradually led up to by Crookes's and many other experiments on cathode rays; and they are shown to be an apparently invisible unit or atom of electricity whose inertia is wholly electric.

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* The argument is plausible, and, taken as an illustration on ordinary lines, will serve; but considered seriously it may be quite fallacious, although the main consequences which in the text are going to be drawn are correct. Few things are more surprising than the extraordinarily large charge held by or constituting an electron in proportion to its size. The charge is so large that ordinary arguments about electricity as it exists on material spheres cannot be expected to apply. If they did, or in so far as they do, the potential of an electron would not be two volts but well over a million volts; and the density of the etherial substance of which it is presumably composed (if its electric inertia is to be derived in any simple ordinary way from its bulk), would have to be nothing like that of water, but of the order 10^{12}, or a billion times the density of water. A thousand tons, in fact, to the cubic millimetre.

We are here out of our depth among quantities on which a great deal of work has to be done to reduce them to order. Yet it must not be supposed that these figures are nonsensical. They require serious consideration; and that is all that can be said for them. I do not think there is any sense in talking about the potential of an indivisible unit of charge, but we can talk about the potential existing at the confines of an atom; and that is a reasonable magnitude, about 14 volts in the case of hydrogen, and not very different for other elements.

But on the other side of the subject everything points to the density of ether being exceedingly high, though perhaps not so high as the above estimate. It must at least be greatly denser than platinum or lead, and probably immensely denser.

A difficulty is often felt as to how ordinary matter like a planet can move through such a medium without friction. Density however does not involve viscosity; the two are disconnected; and resistance to motion would be caused only by viscosity, of which the ether appears to have none. There are many ways, more or less satisfactory, of picturing the perfectly free motion of matter through an exceedingly substantial ether of space; there would be innumerable difficulties in supposing friction and consequent generation of heat. It is quite certain that whatever the ether does it does not dissipate energy. That imperfection belongs to the province of molecularly constituted matter.

The proof of this last statement I can only briefly indicate. It is established by the effect of speed on electric inertia. If an electric charge is moving with something approaching the velocity of light, its inertia increases without limit; and the formula given about 1889 by Heaviside, Thomson, and others, for electric inertia as a function of speed, is, in its very simplest form,

$$m = \frac{2\mu e^2}{3a}\left(1 + \frac{v^2}{2c^2} + \text{higher powers}\right)$$

The velocity of light squared occurs in the denominator, so, before we can observe the increase, enormous speeds are necessary. A cannon-ball, or even the earth in its orbit, is hopelessly slow; and we know no artificial means of getting up such a speed as this last, viz. about 19 miles a second. But fortunately radium does spontaneously what we cannot do, it expels electrons with something less, but not very much less, than the speed of light; and Kauffmann's measure of the mass of these projectiles, thus flying at prodigious velocities, confirms the theory, and removes any doubt as to the reality of purely and wholly electric inertia, for electrons.

Furthermore it was found that the very same electrons can be split off or detached from any or every kind of atom, that there is only one kind of negative electron; and though at first there appeared to be many kinds of positively charged particles, the evidence is tending to the discovery of a single kind of positive electron likewise; so it is natural to suppose that electrons are an essential ingredient in matter. And since they possess inertia, even those which are clearly disembodied electric charges, it becomes possible to surmise that in some sense, or in a certain grouping, they constitute the atom, that they confer upon it the inertia with which we are familiar and that in fact electric inertia is the only inertia that exists.

Electric inertia began as the simulacrum of material inertia, it has shown itself the very same thing, and it seems likely to end by displacing every other kind of inertia altogether.

This is the electrical theory of matter.

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Assuming this theory for the present as a working hypothesis, we may say that material inertia is explained electromagnetically, i.e. is explained in terms of the magnetic field which necessarily surrounds and accompanies every charge in motion; since a charge in motion constitutes a current. For on this view a material body is but an aggregate of such charges grouped according to some definite pattern, positive and negative charges interlaced or somehow intertwined, and so far apart in proportion to their size that they do not interfere with each other or cancel each other, nor apparently overlap or encroach on each other's field, to any measurable extent. Is this possible? It is. For comparing the size of an electron with the size of an atom we perceive that they are relatively of the same order as the size of a planet and the size of a solar system. So it becomes possible to think of an atom as a sort of solar system, with a positive nucleus or sun surrounded by negative electrons revolving in regular orbits round it.

On this view, or indeed in any form of the electrical theory of matter, the atom of matter consists mainly of empty space; in other words, it is excessively porous; just as the solar system is mainly empty space and may be spoken of as excessively porous; the actual material lumps being almost infinitesimal in proportion to the total bulk. A rapid projectile or a ray of light passing through the solar system would be unlikely to hit anything, the chances would be strongly against a collision. So also, if a point be thrown through an atom, the chance of its hitting anything is about 1 in 10,000. It might pass through 10,000 atoms before striking. This experiment has been tried, by C. T. R. Wilson and others, and that is roughly speaking the result. Sooner or later a radium projectile meets with an obstacle and is stopped, but it traverses a good number of atoms on the average; it traverses quite a perceptible distance even in a dense solid, before it strikes a nucleus.

Matter accordingly seems to me—to us I may say, for in this most physicists are I think agreed—a gossamer or milky-way structure, an impalpable accident in the substantial ether. Here a speck and there a speck, but, for the great bulk of it, empty space!

"Impalpable" is not the right word, for matter is essentially palpable. It is because it appeals so directly to our senses that we attend to it so vividly. It forces itself on our attention, while the ether eludes us. And why? Clearly because our bodies are composed—our sense organs are composed—of this very matter. On the material side we are part of, and thoroughly at home in, the material universe. Whereas the ether is elusive; we know nothing of it directly; and though our eyes are instruments for receiving etherial tremors excited by agitated electrons, we only know that fact—or half know it—by rather recondite inference. Light really tells us nothing about its own nature, but only about the superficial aspect of that gross and palpable matter which has interfered with and scattered it before it enters our eye.

Nevertheless the atoms of this solid-seeming flesh and matter as we know it, when analysed into constituents, are turning out to be composed each of a definite grouping of ultra-minute particles, the positive and negative electrons, which themselves hardly occupy any space (save as soldiers occupy a country), and which appear to be of two kinds only—the ultimate indivisible units of positive and negative electricity.

PART II.—THE POSSIBLE STRUCTURE OF ATOMS, AND THEIR RADIATION.

How then are we to explain the different kinds of matter? Here we enter upon territory so recently annexed as to be still very debatable; but progress has been and is still being made, and it is only through the work of recent explorers that we can attempt to answer the question at all. It is invidious to mention names, but I must mention Rutherford, Soddy, Barkla, Bragg, Moseley, Nicholson, and Bohr, among many others. Moseley—as brilliant as any of them, and patriotically self-sacrificing like all our splendid youth—was killed, alas! by a Turkish bullet at Gallipoli; though not before he had made an immortal discovery. How much more might he not have accomplished had it not seemed good to evil powers to impose by force their dominance on the world!

To give a certain and definite answer to questions about the structure of the atom is premature. I can only state the answer which at present tentatively appeals to me, and I think to others. Your Professor of Natural Philosophy (Sir J. J. Thomson) is lecturing I see on Saturday afternoons concerning spectroscopic evidence on this great subject, and he will no doubt carry the matter further.

Meanwhile, and very briefly, the idea about the atom which at present seems most likely to be on the path towards truth is, a central positive nucleus surrounded by a system of negative electrons—so much is pretty certain—while according to one theory the system is composed of revolving electrons, moving under an inverse-square law in regular orbits, very like the sun and planets. The orbital movement is governed by electric force instead of by gravitation, but the laws of motion, and the perturbations which may be caused by outside forces, are very like those familiar to astronomers.

According to Moseley's experimental counting, and Bohr's theory, hydrogen seems to be like a sun with one planet, just a positive and a negative electron; the two being equal electrically, but differing in inertia, the positive being the more massive, though probably for that reason the smaller or more concentrated of the two. Helium seems to have two central unbalanced positive charges and two revolving negative; lithium, three of each; beryllium, four; boron, five; carbon, six; nitrogen, seven; oxygen, eight; and so on, according to the number of the element in Mendeleéff's series—a number something like half the number expressing its atomic weight.

The number of positive atoms in the nucleus was counted, for several elements, by Rutherford; and the number of negative corpuscles in the orbit was counted by Moseley: the two numbers agree. Normal atoms are therefore electrically neutral, so that their external electric attraction at any reasonable distance is nil; but it is supposed that at atomic or molecular distances the outer or orbital electrons, which can interlock with those of others, determine the atom's chemical affinity and all the chemical behaviour of the substance. An atom with one or two outlying planets—let us surmise—would be an active chemical element, a monad or dyad perhaps. An atom with a close-grouped self-contained system would be an inert element of the argon neon helium series. These might exhibit chemical properties, perhaps, under enormous pressure. The heavier atoms contain the most particles, and must have the most complicated structure. There is every grade; from the simplest, hydrogen, with one electron, to the most complex, uranium, with ninety-two. There is room for ninety-two elements in the series, and no more All of these are actually known except five or six. There are only these few unfilled gaps in the chemical series of elements as thus planned.

Radioactivity.

A complicated atom has a certain amount of instability and may fall down occasionally into the next simple grouping, flinging away one or more of its units. When this happens there is a sort of atomic cataclysm or explosion, a projectile and some quanta of energy are emitted. This is the phenomenon of radioactivity. Uranium after three (or possibly four) such eruptions becomes the element three (or four) steps down the series, viz. radium. Radium after five more explosions becomes apparently the well-known and stable element lead, or at least something chemically indistinguishable though perhaps of slightly different atomic weight—what has recently been called an "isotope" of lead. That is the kind of statement that without too much rashness can be cautiously and tentatively made.

At every serious cataclysm an α particle or atom of helium is emitted from the nucleus, accompanied by a β particle or negative corpuscle from somewhere, usually from the planetary system. A sympathetic etherial gush of γ rays accompanies the eruption. A definite unit of energy—a quantum or a simple multiple of it—is emitted at each explosion; and the remaining electrons then settle down into their new orbits, the element changing in character and chemical properties accordingly.

A catastrophe of this kind can be produced by a sufficiently rapid projectile, an α or β particle shot off say by radium; and a minor catastrophe or emission of a β particle can also be produced by the accumulated energy of properly attuned X-rays. When an X-ray or ray of ultra-violet light agrees in frequency with the orbital frequency of an electron, we can suppose (not without a little difficulty) that its energy is absorbed till a quantum has been accumulated, and then a β ray or excessively rapid electron is emitted.

Remarks on the Quantum.

In my view it should not be thought that energy exists in numerical bundles, or quanta ; the discontinuity is not really in energy, but in the atom. Atomic properties are essentially numerical and discontinuous, and we ought not to be surprised at an equilibrium which needs a specific amount of energy to upset it. The energy must be supplied by the disturbing impulse ; but in the case of ultra-violet or X-ray radiation, the energy can only be attributed to the disturbing impulse on the principle of resonant or syntonic accumulation ; for its intensity does not matter. Nor ought it to matter so long as the tuned impulse is repeated often enough—a repetition for which an extremely minute fraction of a second is ample. What matters is not the brightness or energy of the incident radiation therefore, but its frequency. On the other hand, a β projectile cannot effect a real disturbance unless it possesses a minimum quantum of energy ; for in that case there is no accumulation.

The quantum, considered merely as a finite store of energy, is susceptible of exceedingly elementary illustration. Here is a case of stable equilibrium (a simple pendulum, or a round-bottomed flask loaded so as to oscillate stably) which responds to the slightest touch and returns to equilibrium. There is no quantum about that. But here is another case of stable equilibrium (a brick or block or pillar standing on end) which takes no notice of any but a finite force, and which requires a finite amount of energy to upset it—viz. its weight multiplied by the elevation of its centre of gravity as it revolves round its lower edge : this being also the amount of energy emitted when it falls. Or there may be a union of the two kinds of equilibrium. This rounded rocking flask, for instance, or a rocking-horse, may accumulate oscillations till the energy reaches a sort of quantum, when it upsets and breaks or causes an accident. This last is the kind of stable equilibrium which we meet with in an atom.

A flying particle below a certain limit of energy can alter the excentricity of an orbit, and may thus excite some simple radiations which continue till the orbit becomes circular again ; but a synchronous X-ray disturbance, however intrinsically feeble, may precipitate a catastrophe ; and simple facts of this kind seem to be in the main responsible for the general notion of quanta of energy. The really remarkable thing about a quantum, the thing which makes it so essentially worthy of attention, is the fact that it is a universal constant : the same amount of energy is found associated with every kind of matter—the same, or differing only by simple multiplication. Hence the notion, at one time put forward, that energy itself might be atomic, and exist in indivisible packets, like cartridges.

Hypothetical Structure of Atoms.

The real facts concerning the quantum, which are the result of observation, suggest when interpreted properly that there are stable electronic orbits in an atom, and that these follow a regular law of succession, analogous perhaps to Bode's law of planetary distances in the solar system. Spectroscopic evidence — the so-called Balmer's series of lines—strongly bears out this idea. For there is what is called K radiation, of highest frequency, apparently due to perturbations of the innermost, the most rapid, ring ; L radiation of lower frequency from the next outer ring ; M radiation from a ring outside this ; and recently there is talk of a J radiation of extra high frequency from a ring still closer to the nucleus—perhaps quite close to it, part of it perhaps—and anyway well within the K ring.

The frequencies adapted to bring about an atomic catastrophe, or which are emitted during perturbations, are usually high up in the series of X-ray series of vibrations, far above visible light. I assume that these frequencies correspond with the frequency of orbital revolution, and that the inverse-square law holds good. The more massive the nucleus, the greater must be the frequency of orbital revolution at a given distance, in accordance with Kepler's third law. The square of the frequency multiplied by the cube of the radius of the orbit will be constant for all the orbits of all the atoms of any given substance, and will give the attracting force of the nuclear centre for that substance.

In other words, this product (or, what comes to the same thing, the radius multiplied by the square of the speed) will correspond to the number of unneutralised positive charges which go to make up the nucleus. It will give in fact the number of the element in the Mendeléeff series. The K radiation frequency from uranium therefore must be exceptionally rapid, because the nucleus is so strong. For hydrogen, whose nucleus is only 1/92nd of that of uranium, the orbital frequency might be comparatively slow, not higher than the ultra-violet ; while the L radiation from hydrogen, it is now thought, may be within the limits of the visible spectrum ; an M series being perhaps in the infra-red.

But how comes it that hydrogen, with only one electron, can have a K series and an L series and an M series at all ? Bohr's theory suggests that even a single electron may have alternative orbits—not necessarily occupied ; and the spectroscope strongly suggests that the radii of these alternative orbits run as the squares of the natural numbers

$$1 \quad 4 \quad 9 \quad 16 \quad 25 \quad \text{etc.}$$

The frequencies, or reciprocals of periodic times, would then be as the inverse cube of the natural numbers

$$1 \quad \tfrac{1}{8} \quad \tfrac{1}{27} \quad \tfrac{1}{64} \quad \text{etc.,}$$

and this is approximately and roughly what the K L M series of spectrum lines correspond to—with some exceptions.

When a cataclysm occurs and an electron is expelled, it is expelled as I think with the velocity which it possessed in the atom just before it burst its bonds and flew off. For the energy required to fling a planet to infinity, under an inverse-square law, is just double the energy with which it was already moving in its circular orbit. Its own orbital energy is therefore the quantum of energy that has to be supplied in order to get a satisfactory ejection. Some of it might be supplied by the falling in of other particles from their original orbits: for their kinetic energy therein would be inversely as the distance from the nucleus. Hence if K, L, M orbits have the radii 1, 4, 9, three units of L energy would represent the fall from L to K, and this added to the original L energy would give the quadruple L energy which is equal to the K energy, and able to eject a K particle. Similarly a ninefold multiple of the M energy, eight units of which would be acquired by falling to K, would supply that particle with the ejection energy, equally well.

Would an M particle falling to L be able to eject an L particle? $\frac{1}{4} - \frac{1}{9} = \frac{5}{36}$ of a K unit of energy would be acquired in the fall from M to L, that is $\frac{5}{4}$ M units, so altogether $\frac{9}{4}$ of M energy would be transmitted, and that, being equal to a unit of L energy, ought to be sufficient.

Hence, in general, particles may be ejected from any ring, either by direct impact from outside, or by accumulated disturbance of X-rays, or by a collapse of particles from one orbit to the next; and from an immense group of atoms, as in a visible speck of substance, all kinds of radiation can be emitted simultaneously.

Are we to suppose that there is only one electron in each orbit, or may several of them distribute themselves over a ring in accordance with some law of stability? Both alternatives are possible, and both are likely to be found in nature. It seems hardly likely that a uranium atom should possess 92 different orbits, although it does contain 92 electrons. Yet even this number of orbits is possible within the dimensions of an atom. We need not exclude the possibility, as taking up too much room. For given the size of the ultra-innermost or J orbit as 1, the outer orbit would, on Bohr's law pressed to extremes, be $(92)^2$ times that radius, say 8464 times the size of the innermost orbit; but if this innermost orbit is near the uranium nucleus, which may be $\sqrt[3]{92}$, or say 5 times the radius of the hydrogen nucleus, the boundary or confine of the atom is some 10,000 times as far away; leaving therefore just room enough for the 92 Bohr orbits, though not much more than is required.

Hence if there were any reason to desire them separate, they could be made room for, without endowing the atom with outlying or ever-ready electrons likely to confer upon it very active chemical properties. But so far as I see, so many separate orbits are not likely; for there is every probability that periodically, as you ascend the series, the outer ring is not occupied by a single electron but by a closed compact sort of structure of many electrons, with very little outside affinity, so that we reach periodically an atom which is chemically inactive, helium, neon, argon, krypton, etc., up to emanation, or what Ramsay called niton. So little cohesion holds between such atoms that they are able to exist as permanent gases in spite of the high density of some of them. This at least appears to be the view of Rutherford and Soddy. Helium only condenses to a liquid when cooled down to near the absolute zero of temperature. Its cohesion or intermolecular attraction is nearly nil.

A Fanciful Analogy.

If I attempt to compare the supposed alternative orbits in an atom with the known orbits of the solar system, it is mainly to emphasise, provisionally and tentatively, and perhaps semi-humorously, the astronomical view of the atom; and to bring out still more strongly the resemblances, whatever thorough differences there may be as well.

I write down the squares of the natural numbers, therefore, and underneath put the names of planets, with its real distance written under each in the same units.

Radii of Bohr's atomic orbits	1	4		9	16	25	36	49	64	81	100	121	144	169	196
		m	V	E	M	Asteroids		J			S				U
Planetary distances		3·9	7·2	10	15·2	20–35		52			95·4				192

The obvious suggestion is that asteroids should be looked for between Jupiter and Saturn, and between Saturn and Uranus; but I would not venture to predict the existence of any such bodies on the strength of this analogy, because you will doubtless have noticed that no analogue of the planet Venus appears in the list of atomic orbits: the scheme provides no place for her—a lamentable omission which must discredit and I expect condemn even the analogy. Nevertheless I make no apology for introducing it, in order to emphasise astronomical similarities in the possible structure of an atom.

QUANTITATIVE INTERPOLATION.

On Atomic Radiation.

Permitting ourselves this view of the atom as a working hypothesis, we have to picture each atom as an attracting centre or nucleus, with a number of alternative orbits in regular succession round it, but not all necessarily occupied by revolving electrons.

The atoms of different elements differ in the number of positive units in the nucleus, and in the corresponding number of revolving negative units; in fact the diverse chemical elements, in their atomic constitution, form a definite arithmetical series with common difference one. There is a discontinuity or finite step in passing from one element to the next in the series, there is no continuous passage from one to another; hence if the physical transition or mutation ever occurs, it must be by some sort of sudden convulsion.

To extract laws for this hypothetical structure, suggested by the labours of many workers, we may attend to the different rings of one kind of atom, or we may attend to the corresponding rings in different kinds of atom. Each, for instance, has an innermost ring, which it is convenient at present to call the K ring, because of the shortest wave lengths, or so-called K spectrum, which its perturbations emit. And, in ascending the series of elements, as the nucleus gets stronger by addition of units, the electron in this innermost or K ring must revolve faster and faster to counterbalance the greater attracting force. Its orbit will accordingly get smaller and smaller, in the proportion proper to the law of inverse square. And the frequency will increase for both reasons, i.e. for both the greater speed and the shorter journey. The spectrum accordingly, while preserving the same type, ascends the ladder of frequency.

Suppose the atomic number, or strength of the nucleus in atoms of successive elements, increases in arithmetical progression $N - 1$, N, $N + 1$, etc., then the radius of the given type of orbit may shrink in the same proportion, so that rN is constant; and the velocity v may increase in the same proportion, so that rv is constant; or in other words so that the moment of momentum in corresponding rings of different atoms is the same. There is good evidence that such is the case. The law, so far as it is a law, is styled by Professor Millikan, the atomicity of angular momentum. If the value of mvr or $mr^2\omega$ differs in different rings, it differs by finite steps.

The frequency of orbital revolution will depend on v directly and on r inversely, so the frequency $(v/2\pi r)$ will increase in the proportion of N^2; and this, in some form or other, is known as Moseley's law.

The energy, $\frac{1}{2}mv^2$ in a given type of ring, will also depend upon N^2 in different atoms, and is therefore simply proportional to the frequency. The orbital energy is half the energy with which a particle breaks loose (or is driven to infinity) whenever a convulsion occurs. The convulsion can be stimulated by X-rays or ultra-violet light of the right frequency; their energy appears to be stored by resonance until the critical breaking-up point is reached. The ratio of emission energy to frequency is a remarkable universal constant, and is called h, the quantum. It is not energy, but the accumulation or integral of energy for a certain time; and it is permissible to write $m v^2 = 2 m u^2 = h n$; because the emission velocity u (the velocity from infinity) is $\sqrt{2}$ times the orbital velocity v. But h, or rather $h/2\pi$, may also be taken as representing the orbital angular momentum, $m v r$ (more strictly, if the orbit is at all elliptical, $m v p$) for the ring whence the particle came. It would be rather convenient if the designation h were transferred to $h/2\pi$ before it is too late; but I must leave this minor change to the approval of leaders in this subject.

I may point out that this constancy of angular momentum in different orbits bears a curious analogy to Kepler's second law about rate of description of areas in the same orbit. And, if a coincidence, it is odd that the symbol h should have been used both for Kepler's $r^2 d\theta/dt$ and for an atomic quantity which is also $r^2 d\theta/dt$ multiplied by $2\pi m$.

Within each atom Kepler's laws must presumably hold; so r^3/t^2, or $r v^2$, is constant for the different circular orbits in each atom; whence the energy in successive rings of one atom is inversely as their radii; hence the ring most likely to eject a particle is the innermost or K ring.

This characteristic constant $r v^2$ of an element is proportional to the central attracting force, and therefore proportional to N. Hence it goes up step by step in the series of atoms, as N does.

Summary.

N is Moseley's atomic number, and equals the number of orbital electrons, or the number of unbalanced positive charges in the nucleus. The constant $r v^2$ is characteristic of all the rings in one atom (N being constant). The product $r v$ is a constant characteristic of a given type of ring in the whole series of atoms (N going up step by step); but in any one atom this product $r v$ ascends from ring to ring in regular arithmetical stages, the same stages as $\sqrt{r}$.

The product $r v^2$ is constant inside each atom and proceeds by steps from atom to atom; while the product $r v$ is the same for different atoms, but changes inside each atom and proceeds by steps from ring to ring. In fact we may write :—

For all the rings in one atom.

Central force	$r v^2$ is constant.		
Angular Momentum for the rings in one atom	$r v$	$\propto$	$\sqrt{r}$
Energy for the same . . .	v^2	$\propto$	$1/r$

For any ring in any atom.

Central force for any ring in any atom	$r v^2$	$\underline{\propto}$	N

For the same type of ring in different atoms.

Radius of given type of ring in any atom	r	$\propto 1/N$
Orbital Velocity in ring of that type .	v	$\propto N$
Moment of Momentum in given type of ring	$r\,v$	is const. as regards N.
Frequency in that type of ring . .	v/r	$\propto N^2$
Energy in same	v^2	$\propto N^2$

So for a given type of ring in different atoms the orbital energy is proportional to the frequency; which is a curious result thoroughly consistent with Moseley's law, ascertained by experiments on emission, and true at any rate for emission energy.

The ratio

$$\frac{\text{emission energy}}{\text{frequency}} = \frac{m\,v^2}{v/2\,\pi\,r} = 2\,\pi\,m\,v\,r = 2\pi . m\,r^2 . 2\,\pi\,r = 2\,\pi\,I\,\omega$$

So if we call this h, or a multiple of h, then on our hypothesis $h/2\,\pi$ is the indivisible unit of angular momentum for an orbital electron.

The speed with which an electron is ejected is very high, something like 0·9 of light, so the increase of mass at high speeds must be taken into account in propounding a reason for the emission of corpuscles.

Radiation Heterodoxy.

In considering the radiation from an atom I have virtually made the hypothesis that so long as orbits are circular they do not radiate, but that if perturbed into ellipses, with corresponding fluctuation of speed—as they would be by the influence of a flying charge passing through or near them—then they would radiate, with the proper orbital frequency, until the excentricity disappears again and they resume their stable circular orbit once more. Though of course they might be so much perturbed as to eject a particle. Any one of the rings, if perturbed at all, may radiate and give appropriate spectral lines. An external synchronous alternating field will also cause them to absorb energy, even though they were not radiating any until the extra energy arrived.

This hypothesis, if at all regarded, is equivalent to a request to mathematicians to reconsider their theory of electronic radiation. Radiation intensity is known to be proportional to the square of acceleration (Sir Joseph Larmor, and to some extent FitzGerald and Hertz, established this), and I must admit that the reasoning seems to make this law applicable to every kind of acceleration; but my rash suggestion is that it may be only speed-acceleration that is really effective, and not transverse or curvature-acceleration at constant speed. For this will not perturb the lines of force holding the electron to the nucleus, but will leave them in a constant condition, so long as the orbit is circular and the speed therefore constant. There is a recognised difference of the same sort in connection with varying inertia; its value is not affected by transverse acceleration, with the speed left constant, but it is affected by longitudinal acceleration, which alters the speed.

So I am in hopes that it may be found that this latter or speed-acceleration is what is responsible for radiation, and that mere curvature at constant speed in a circular orbit need not radiate at all. Provided always that the superposition of an external alternating field of the right frequency may cause absorption. Many of the difficulties connected with the stability of the astronomical atom would be evaded if the theory of radiation could be modified in this way; and the excitation of characteristic radiation by almost any kind of perturbation of the orbit would be intelligible.

Speculations on Radiation and Atomic Structure.

Bohr's remarkable theory of atomic structure does not pretend to be strictly dynamical; it is partly empirical, being based on the discontinuity signalised by Planck's constant, but it is very brilliant, and extensively justifies itself by agreement with facts.

His expression for the frequency of radiation emitted by any element is virtually, to a fair approximation—

$$n = \frac{2\pi^2 me^4}{K^2 h^3} \cdot \left(\frac{E}{e}\right)^2 \left(\frac{1}{p^2} - \frac{1}{q^2}\right)$$

where $\frac{E}{e}$ is Moseley's atomic number, N the number of unbalanced charges in the nucleus or the number of electrons in the atom, and where p and q are integers of which p changes from series to series, while the lines in each series are given by the mutations of q. For heavy atoms the E in the above formula should be E minus a geometrical function of all the other electrons inside the radiating orbit, because they will affect the central attracting force. In this way outstanding discrepancies may plausibly be explained. But the remarkable thing is that the formula gives the frequencies not merely relatively but absolutely. For if the experimental values otherwise obtained for e, m and h are inserted, the constant outside the brackets, called Rydberg's constant, which is spectroscopically determined and known to be the same for all elements, comes out right. A very notable fact!

The above expression for spectral lines not only agrees with the Rydberg-Balmer known spectroscopic series, and with the kind of formula given by many pioneer workers, but has been able to predict other series which have been subsequently observed. It also accounts for many extra-low-frequency lines which though not obtainable in the laboratory are observable astronomically, by suggesting that they come from very large masses of highly rarefied gas. For under such conditions the atoms would have more room and could possess far outlying or ultra-Neptunian electrons, and yet have total substance enough to display their spectra.

To contemplate the emission of radiation, both waves and particles, we may picture one of the satellite electrons in a many-orbited atom struck or so thoroughly perturbed by the sudden arrival of a foreign charge as to precipitate it into the next inner ring, ejecting the constituent of that ring into the one below, and so on, after the manner of the "jack for mustard" game with a series of wooden bricks set up on end.

Wave emission should accompany each transition. The effect of precipitating the innermost electron on the body of the nucleus is not clear; but a compound nucleus must be a strangely interlocked conglomerate, and an explosion seems not unlikely: especially if one of the supposed binding negative electrons were ejected. The potential gradient close to a nucleus is prodigious.

The effect of the arrival or departure of a charged particle at the nucleus would be suddenly to change its intrinsic attracting force; and this of itself would render all the orbits elliptical for a time, with excentricity $\frac{1}{N \pm 1}$,[*] thus exciting radiation of several frequencies. If the radiation ceased when the excentricity was got rid of, a new circular orbit would be taken up, and thus perhaps discontinuities might be accounted for in a dynamical manner.

The effect of properly attuned X-rays or ultra-violet light, if it is to be accomplished through resonance—and it is difficult to account for its

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[*] This can be proved as follows:—
For a circular orbit

$$r v^2 = \mu$$

and

$$r^2 v^2 = h^2 = \mu r.$$

When μ suddenly changes to $k \mu$ $\left(\text{where } k \text{ may be } \frac{N \pm 1}{N}\right)$ the velocity does not instantly change, but the orbit acquires an a and an e, such that

$$a(1 - e^2) = \frac{h^2}{k \mu} \text{ or } e^2 = 1 - \frac{r}{k a};$$

also

$$\frac{\mu}{r} = v^2 = k \mu \left(\frac{2}{r} - \frac{1}{a}\right).$$

This last gives

$$\frac{r}{a} = 2 - \frac{1}{k},$$

so

$$e^2 = 1 - \frac{1}{k}\left(2 - \frac{1}{k}\right)$$

$$= \left(1 - \frac{1}{k}\right)^2.$$

And the place where the sudden impulse occurred becomes an apse of the new orbit, because $r = a(1 \pm e)$. (*See also Appendix I.*)

independence of intensity otherwise—seems to require a fair range of frequency in those rays; for their effect on a revolving electron would naturally be to increase its angular speed and so throw it out of tune with the particular disturbance to which it initially responded. The sectorial area swept out would increase, the radius vector would increase, the linear speed would therefore diminish in spite of the resonant effort to increase it. Unless indeed, under the peculiar conditions in an atom, there may be some compromise. The alternative would be for the electron to be constrained, under conditions of stability, to maintain its frequency unaltered, either proceeding in an outward spiral towards a position of Planckian instability, or trying still to obey the law of inverse squares by increasing the excentricity of its orbit with given axis major until it becomes practically parabolic. (*See Appendix II.*)

This could represent an inversion of the process by which the electron may have been originally bound, according to Bohr's theory of what happened before the atom became neutral. For it is to be presumed that a positively charged α particle, after ejectment, neutralises itself by accretion and settles down.

CONCLUSION.

I have led you over a great deal of territory in a hurried manner, and occasionally have entered on regions where the ground is not yet solid and secure. Let it be granted that the chemist may naturally object to an astronomical atom and may prefer a more static or geometrical structure, although such a structure would have less clear and explicable properties. The static or Boscovich atom, with purely hypothetical interior fluctuations of force, leaves everything in the dark, and is therefore less tempting to a physicist, until some physical explanation of those fluctuations can be given. At present they seem to be postulated merely in order to secure positions of equilibrium in which an electron can settle down. Orbital revolution achieves the same end, in apparently a more complicated but really a more tractable manner. Moreover it confers upon an atom the sort of energy and structural velocities which are conspicuous in the various types of radio-activity. True, it is a working hypothesis at present, and nothing more, but it seems likely to be a fruitful one; and that is its present justification.

The subject is in the nascent or rapidly growing stage; and, provided we refrain from dogmatism, it is legitimate thus tentatively to survey and explore the boundary between knowledge and ignorance, and to speculate as to what may be the next stages in the exhilarating pursuit.

The apparent resemblance between an atom and the solar system opens up extraordinary vistas for further enquiry. Optics and gravitation still have many secrets. The interactions between ether and matter are as yet barely understood. We know that the energy of an electric current is really in the ether, i.e. in the magnetic field surrounding the current; but we must admit that the electromagnetic explanation of inertia is no ultimate explanation—it is but relegating the property to some fundamental property of the ether; of which substance presumably matter itself may in some way be composed.

Evidence suggests that the ether is an excessively dense substance, and that it circulates slowly along lines of magnetic force.* But though so dense we have no means of apprehending it directly. Matter, though so comparatively filmy and fragmentary, yet looms large in our estimation because of our material sense organs; its properties force themselves on our attention because in fact our bodies are composed of matter. But underneath and behind all the known properties of matter lie the unknown properties of the ether of space; and if we are to create a true philosophy we must attend continually to Ether as well as to Matter in the physical universe. The ether makes no appeal to our senses, but it is none the less real for that. Sensation is no test of reality: many of the most important things are in the insensible universe; and he is the wisest man who shuts the door on no opportunity for investigation, but keeps his mind open, and is ready to explore every avenue towards truth.

[O. J. L.]

APPENDIX I.

EFFECT ON ELECTRONIC ORBITS OF SUDDEN NUCLEAR CHANGES.

Continuing the subject of the footnote on page 473, and taking the case of an atom degrading by radioactivity, though an inverse process of ascension by building up light atoms into heavy ones can be equally dealt with if that process ever now occurs: let us assume that a nucleus expels first a negative unit and then a positive unit. At the first emission the nucleus, with N unbalanced positive charges, suddenly gains force in the ratio $k = \frac{N+1}{N}$.

The result on its family of revolving electrons is to make every circular orbit elliptical. With the usual Tait and Steele notation, initially $R V^2 = \mu$. When the change occurs the semi-major axis of the new orbit is given by

$$\frac{1}{a} = \frac{2}{R} - \frac{V^2}{k\mu} = \frac{1}{R}\left(2 - \frac{1}{k}\right);$$

hence

$$a = \frac{N+1}{N+2}R, \text{ and } e = \frac{1}{N+1}.$$

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* This view of the energy of a magnetic field, that it is direct kinetic energy of the ether moving longitudinally, suggests a possible (or nearly impossible) experimental means of determining the real density of the ether of space—a subject on which I have much more to say. See probably Phil. Mag. for May, 1919.

The next disturbance—the emission of an a ray and another β ray—may be expected to occur, by preference, when the quasi-tidal power of the electron is greatest, i.e. when it is at the nearest apse, or when several of the innermost particles are in conjunction there. Granting this for the moment, the velocity of any one of them at that place will be

$$V'^2 = k\mu\left(\frac{2}{R'} - \frac{1}{a}\right) = \frac{k\mu}{a} \cdot \frac{1+e}{1-e}.$$

With this speed, or something near it, the particle is projected into its final orbit, the central force being now restored to its old value, though the atomic weight has gone down a step. The semi axis major of the final orbit is independent of the direction of projection, and is given by

$$\frac{1}{a'} = \frac{2}{R'} - \frac{V'^2}{\mu},$$

whence

$$a' = \frac{a(1-e)}{2-k(1+e)} = \frac{N^2 a}{(N+1)(N-2)} = \frac{N^2}{N^2-4}R.$$

The frequency is inversely as the 3/2 power of a', the excentricity is $e' = \frac{2}{N}$, and the new radius differs from the old one by the ratio

$$\frac{N^2}{N^2-4};$$

an expression which looks as if it ought to account for Balmer's series of spectrum lines.

APPENDIX II.

EFFECT OF SYNCHRONOUS ULTRA-VIOLET OR X-RADIATION.

Referring to pages 465, 470 and 474, about storing a feeble disturbance by resonance, the equations of planetary motion are

$$\ddot{x} = -\mu x/r^3 \text{ and } y = -\mu y/r^3.$$

Upon this we have to superpose a train of waves of very small amplitude

$$y = \kappa \cos \frac{2\pi}{v\lambda}(x - nt),$$

where n is to be the "mean motion" of the planet, $n = \sqrt{\left(\frac{\mu}{a^3}\right)}$.

The essential part of this can be presented as $\ddot{y} = -n^2 y$ to be added to the main term. Hence the equations become

$$\ddot{x} = -\frac{\mu x}{r^3};\ \ddot{y} = -\mu y\left(\frac{1}{r^3} + \frac{1}{a^3}\right), \text{ and } r^2\frac{d\theta}{dt} \text{ is the integral of } -\frac{\mu x y}{a^3}.$$

On submitting these equations to Mr. C. T. Preece, Mathematical Lecturer at Birmingham University, he dealt with them thus:—

$$\dot{x}^2 + \dot{y}^2 = \frac{2\mu}{r} - \frac{\mu}{a^3} y^2 + \mathrm{C},$$

or choosing the constant suitably—

$$v^2 = \mu\left(\frac{2}{r} - \frac{1}{a}\right) + \frac{\mu}{a^3}(\kappa^2 - y^2).$$

The second term represents departure from elliptic motion by the planting on it of a small harmonic disturbance.

If the disturbance ceases at any point (x, y) the particle will continue in an ellipse, such that—

$$a' = \frac{a^3}{a^2 - \kappa^2 + y^2}.$$

The effect of the disturbance, while it lasts, is to shut up the ellipse spirally in the direction of its minor axis, leaving the major axis considerable, the excentricity of the orbit becoming great. In the limit the semi-minor axis might be reduced to κ, in which case $e = \sqrt{1 - \left(\frac{\kappa}{a}\right)^2}$, and the possible range of a' would be between a and a/e^2. So practically e becomes nearly 1 and a' remains nearly a; thus keeping the frequency constant.

The effect of this narrowing down of the ellipse is to bring the particle within the range $\kappa\sqrt{\frac{1-e}{1+e}}$ of the central nucleus, or practically $\frac{1}{2}\frac{\kappa}{a}$—a proximity likely to cause disruption and conferring on the particle a high maximum velocity $na\sqrt{\frac{1+e}{1-e}}$, or approximately $\frac{2na^2}{\kappa}$.

This looks as if we could thus reckon an exaggerated upper limit to the amplitude of light-wave concerned. With ordinary electric value for m it comes out $\kappa = \sqrt{(6\,\mathrm{N}\,a\,z)}$, where z is the size of an electron, and a the size of its orbit; but if m is allowed to increase with speed, β being the ratio v/c, this expression for the minor axis of a disruptive orbit, $\sqrt{(6\,\mathrm{N}\,a\,z)}$, must be multiplied by the fraction $\sqrt{(1-\beta^2)}/\beta$. It seems anyhow to be of the order 10^{-10} centimetre.

GENERAL MONTHLY MEETING,

Monday, March 3, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Hon. Richard Stafford Cripps,
Major Gilbert Dennison,
Major Charles de Roemer,
Miss Marjorie Ruth Dreschfield,
William Henry Eccles, D.Sc.
Francis H. A. Engleheart,
Laurence Francis Alexander Fogarty,
William Adams Frost, F.R C.S.
Athelstan Argyle Hall,
Sidney Frederic Harmer, Sc.D. F.R.S.
Alwyne Meade,
Gerald Newgass,
Edward Yelf Radley, B.A.
Eric K. Rideal,
Robert E. Slade, D.Sc.
William Symington,
Miss Emily G. Western,
Glynne Williams,
Miss Marguerite Xavier,

were elected Members.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Bulletin of Agricultural Research Institute, Pusa, No. 84. 1918. 8vo.
Memoirs of the Department of Agriculture: Botanical Series, Vol. IX. No. 5. 8vo. 1918.

Accademia dei Lincei Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Mathematiche e Naturali. Vol. XVII. 2° Semestre, Fasc. 11–12; Classe di Scienze Morali, Serie Quinta, Vol. XXVII. Fasc. 5–6. 1918. 8vo.

American Geographical Society—Geographical Review, Dec. 1918. 8vo.

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Bankers, Institute of—Journal, Vol. XL. Part 2. 8vo. 1919.

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British Architects, Royal Institute of—Journal, Vol. XXVI. No. 4. 8vo. 1919.
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British Dental Association—Journal, Vol. XL. Nos. 3-4. 8vo. 1919.
Carnegie Institution—Contributions from Mount Wilson Solar Observatory, Nos. 150, 158-159. 8vo. 1918.
Communications to National Academy of Sciences, No. 54. 8vo. 1918.
Chemical Industry, Society of—Journal, Feb. 1919. 8vo.
Chemistry, Institute of—Proceedings, 1919, Part 1. 8vo.
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Dickinson, Miss M., M.R.I. (the Authoress)—The Dickinson Newly-Discovered Radio-Activity. 8vo. 1919.
Drummond, J., Esq. (the Author)—The Canadian Snowshoe. 8vo. 1919.
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Editors—Aeronautical Journal for Dec. 1918. 8vo.
Athenæum for Feb. 1919. 4to.
Author for Feb. 1919. 8vo.
Chemist and Druggist for Feb. 1919. 8vo.
Church Gazette for Feb. 1919. 8vo.
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Electrical Times for Feb. 1919. 4to.
Engineering for Nov.-Dec. 1918. fol.
Ferro-Concrete for Jan. 1919. 8vo.
General Electric Review for Jan. 1919. 8vo.
Horological Journal for Feb. 1919. 8vo.
Junior Mechanics for Feb. 1919. 8vo.
Law Journal for Feb. 1919. 8vo.
London University Gazette for Feb. 1919. 4to.
Metal World for Feb. 1919. 8vo.
Model Engineer for Feb. 1919. 8vo.
Musical Times for Feb. 1919. 8vo.
Nature for Feb. 1919. 4to.
Physical Review for Jan. 1919. 8vo.
Science Abstracts for Jan. 1919. 8vo.
Zoophilist for Feb. 1919. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LVII. No. 278, Jan. 1919. 4to.
Firenze, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani, No. 210, Nov.-Dec. 1918. 8vo.
Franklin Institute—Journal, Jan.-Feb. 1919. 8vo.
Geographical Society, Royal—Journal, Feb. 1919. 8vo.
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Greenewalt, Miss M. Hallock (the Authoress)—Light: Fine Art the Sixth. 8vo. 1918.
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Ponting, H. C., Esq. (*Part-Author*)—Spitzbergen: its History, Strategic Value, Climate, and Mineral Resources. 8vo. 1918.

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Arkiv: Matematik, Astrooomi och Fysik, Band XI. No. 4; Band XII.; Band XIII. Nos. 1-2; Kemi, Mineralogi och Geologi, Band VI. Nos. 4-5: Band VII. No. 1; Botanik: Band XIV. No. 4; Band XV. Nos. 1-2; Zoologi, Band X. No. 4; Band XI. Nos. 1-4. 8vo. 1916-18.

Arsbok, 1917-18. 8vo.

Meddelanden från K. Vetenskapsakad Nobelinstitut, Band III. No. 4. 8vo. 1918.

Register, 1826-1917. 8vo. 1917.

Tasmania, Royal Society of—Papers and Proceedings for the Year 1917. 8vo. 1918.

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Washington, National Academy of Sciences—Proceedings, Vol. IV. No. 12; Vol. V. No. 1. 8vo. 1918-19.

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WEEKLY EVENING MEETING,

Friday, March 7, 1919.

Sir William Phipson Beale, Bart., K.C., Vice-President,
in the Chair.

Professor H. C. H. Carpenter, F.R.S.

The Hardening of Steel.

The capacity of steel for hardening by being quenched from a bright red heat in water is the most important property possessed by any metallic substance. This property is utilized practically in the arts in a great variety of ways, and is the basis of all modern engineering work. To take two types of application only—

(1) It is utilized in the great variety of tools which are employed in modern engineering work, for machining metals and alloys to a high degree of accuracy so that they may constitute a given part of one of the thousand and one machines employed in the mechanic arts of modern civilization, e.g. the locomotive, turbine, gas engine, electric motor, etc.

(2) The razor, the balance spring of the chronometer, the knife, the needle, the pair of scissors, the surgeon's lancet and the dentist's twist drill, are instances of the utilization of this property in the finished steel as well, and they depend upon the capacity of such material to retain its hardness in the absence of stress indefinitely at the ordinary temperatures.

Not only, however, is this property of the greatest practical import, but of the highest scientific interest, inasmuch as the search for the explanation of the capacity of steel for hardening has given rise to a large number of scientific investigations, which have done more than anything else to throw light on the constitution of steels and metallic alloys generally, and have helped to establish the modern science of metallography.

My object this evening is to trace rapidly the history of some of the salient features of this scientific work, and to bring to your notice the most modern views as to the scientific explanation of this wonderful property of steel. I must preface my remarks, however, with this warning—that within the compass of an hour's lecture it is not possible to attempt a complete exposition of the theories relating to the great variety of steel alloys that are now used in the arts, and I shall be obliged to confine my thesis to one of the simplest, albeit the best known of these materials—namely, what is called the pure carbon steel turning tool.

Let us begin by considering the manufacture of the tool itself, and afterwards the conditions under which it is put to work and what follows then.

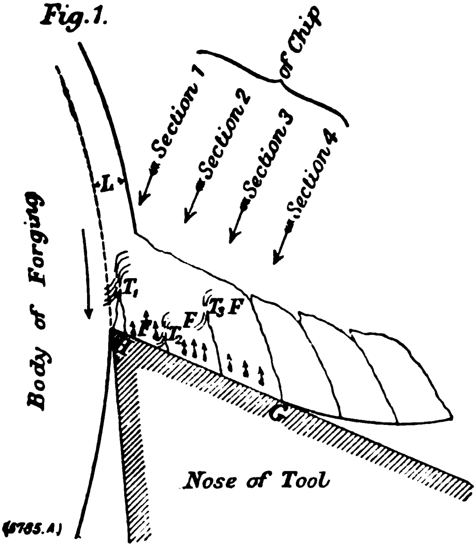
Although even the purest iron containing only the merest trace of carbon can be hardened to some extent by quenching from a high temperature, a certain minimum percentage of carbon is necessary before the hardening is sufficiently marked to confer what may be called practical hardening properties on the steel. This minimum is about 0·7 per cent, and the cutting tools used in the arts range from this figure up to 1·5 per cent. For reasons which will be subsequently apparent, it is simplest to consider the case of a steel tool containing 0·9 per cent carbon. The operation of manufacture of such a tool is briefly as follows: In the first place, a charge designed to give the correct composition is melted down in a crucible in a furnace whose temperature ranges from 1500°–1600° C. The steel is cast from the correct temperature into a metallic mould, giving what is called an ingot, e.g. a rectangular bar about 2½ inches square and 2 feet long. The ingot is subsequently forged down to a bar, say 1¼–1 inch in diameter; it is then cut up into lengths suitable for the dimensions of the tool itself, and next it passes to the hands of the smith, who forges it by hand and fashions the tool to shape. During this operation it is of the greatest importance that the composition of the steel should be altered as little as possible by oxidation of the carbon, otherwise the tool will not harden properly or evenly. Then follows the actual hardening operation, in which the nose of the tool, as it is called, is carefully heated to a given temperature in the neighbourhood of 800° C., withdrawn either from the smith's fire or, better still, the hardening bath, and quenched outright in a bath of cold water at the ordinary temperature.

This is an operation requiring the utmost skill. If the surface of the metal has been decarbonized in heating and scale has formed on the surface, the rapid transference of heat from the metal to the bath will not take place, imperfect hardening will result, and very likely cracks will be formed. If, however, the tool, as in the best modern practice, is heated up in a bath of fused salts, no loss of carbon takes place, and when it is removed from the bath it is covered with a thin film of fused salt; this dissolves almost instantaneously in the water, and the necessary rapid transference of heat from the metal to the water takes place. The best quenchings always have a peculiar "bite," accompanied by a dull, albeit sharp, sound as the large bubbles of steam generated by the heat are absorbed in the surrounding water.

The steel thus hardened, although possessing the necessary hardness, is too brittle to be used as a tool; accordingly the tempering process follows, in which it is heated to a moderate temperature depending on the work to which it is to be put; this temperature varies usually from 200°–300° C. This tempering process, while it withdraws some of the hardness produced by quenching, confers a most valuable property on the tool, namely, toughness, by means of which it stands up to its work, at any rate for a time, without cracking. Then comes the last stage, namely, the grinding of the tool on the grindstone, whereby a clean cutting edge of the required shape is produced; the tool is then ready for use.

Let us suppose that it is to be used in taking a cut from a cylindrical bar of an unhardened steel. The latter is fixed in a lathe and rotated at an appropriate speed (Fig. 1). The tool, held in a toolholder, actuated by suitable mechanism, is gradually brought up to the end of the rotating bar and a given rate of feed maintained. For an instant there is actual contact between the cutting edge of the tool and the bar; the moment, however, that this happens a chip is formed and a shearing stress is set up, as a result of which the work falls not on the actual edge of the tool, but on an area inside. If the metal which is being machined is ductile, it is cut away in long shavings whose thickness depends on the feed; if, on the other hand, it is brittle, it breaks off into short chips. The effect of this friction between the two metals, and the pressure of the shaving, is to generate heat, and to raise the temperature of the tool to a much greater extent than that of the bar which is being machined, and

Fig. 1.



deterioration of the tool sets in ; its hardness gradually diminishes, and *pari passu*, wear of the tool itself by abrasion occurs. Eventually a stage is reached at which the cutting operation has to be discontinued, and the tool requires re-hardening or re-grinding. The carbon tool of which I have just spoken cannot be used for taking the heavy cuts of which the modern alloy tools are capable, but they retain their pre-eminence even to-day in all machinery work where the highest degree of accuracy attainable is desired.

A complete theory of the action of such tools must account for both the hardening and toughening of the steel, and its gradual loss of these properties, by the operations I have described.

Before attempting this, however, it may be interesting to refer briefly to the methods of hardening which chiefly occupied the attention of early workers.

The Greek alchemical manuscripts give various recipes, from which it is clear that in the early days the nature of the quenching liquid was considered to be all-important. There were certain rivers the waters of which were supposed to be specially efficacious, and Pliny mentions that the difference between waters of various rivers can be recognized by workers in steel. Many old recipes for hardening and tempering have been lost, but a number of them have come down through the ages, and from them I take the following illustrations. The first is from a work entitled "Rechtegebrauch & Alchimei," 1531, of which an English translation appeared in 1583. "Take snayles and first-drawn water of a red die, of which water, being taken in the first two monthes of harvest when it raynes, boil it with the snayles, then heate your iron red hot and quench it therein, and it shall be as hard as steele." "Ye may do the like with the blood of a man of XXX years of age and of a sanguine complexion, being of a merry nature and pleasant, . . . distilled in the middst of May." It would be interesting to know the circumstances which led to this particular experiment being first tried. These instructions may seem trivial, but a belief in the efficacy of such solutions has continued, for in a work published in 1810* the artist is directed to take the root of blue lilies, infuse it in wine and quench the steel in it, the steel will be hard. On the other hand, he is told that if he takes the juice or water of common beans and quenches iron or steel in it, it will be soft as lead. When the practice of an art is purely empirical it is liable to take fantastic forms; even at the present day, however, there are many workshops where steel is hardened in which some quaint nostrum still holds sway. Occasionally, but not often, the use of these compounded baths was supported by theoretical views. Otto Tachen, for instance, writing of steel about the year 1666, says that steel "when it is quenched in water acquires strength because the light alcaly in the water is a true comforter of the light acid in the iron, and cutlers do strengthen it with alcaly of animals" —hence the use of snails.

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* "The Laboratory or School of Arts."

Though the practice and theories in the periods I have mentioned were frequently fantastic, these early workers were quite right in attaching importance to the liquid in which the steel was to be quenched. To-day much simpler methods are used in the best modern practice. If severe quenching is required, cold water or brine is used; if a lesser degree of hardening is necessary, hot water or oil; while in many cases the processes of hardening and tempering are combined in one operation by plunging the tool in a bath of molten metal such as lead.

The rapid changes of temperature which occur when a bar of steel is quenched in a liquid from a high temperature have been studied experimentally by Le Chatelier, Benedicks and Lejeune, and their results utilized by McCance in calculating the theoretical curve of quenching. Le Chatelier found that water gave a more rapid rate of cooling than any other liquid, and concluded that the most important property in determining the quenching power of a liquid was its specific heat. Benedicks, by means of an automatic quenching and photographically recording apparatus, obtained the most accurate results which are available as to the temperature at the centre of small round bars quenched in water. On the whole his conclusions corroborated those of Le Chatelier, but he showed that the latent heat is the most important property in determining the quenching power of a liquid under practical conditions.

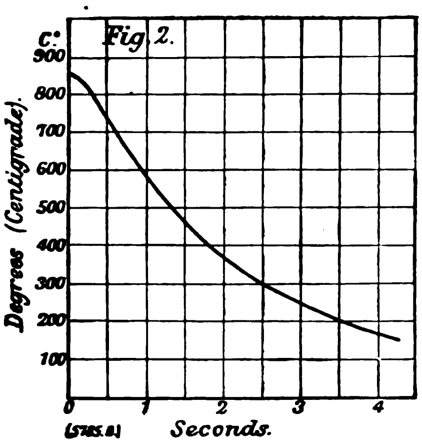
Fig. 2.

The appended cooling curve (Fig. 2) deduced by McCance from Benedicks' results shows what happened when a bar of steel containing 1 per cent carbon, 2 inches long and ¼ inch in diameter, was quenched from 850° C. in water at 15° C. This curve gives the temperature at the centre of the bar at any given moment. It will be observed that it does not fall for the first fraction of a second; that the rate of fall starts slowly, then increases rapidly to a maximum, afterwards decreasing more and more slowly until it reaches the temperature of the surrounding water. From our present point of view the important thing to notice is that the time from the moment of immersion until the temperature had dropped to 500° C. was 1¼ second. As the diameter of the bar increases, the rate of chilling is of course diminished, and no doubt in the case of a rectangular turning tool of 1⅛-inch diameter several seconds would be required for the same drop in temperature at the centre. As McCance has pointed out, the perfect theoretical conditions for quenching are "that a specimen heated uniformly has its surface suddenly cooled to a lower temperature and kept at that lower temperature without alteration until it has once more obtained uniformity, but at the lower temperature; the temperature of the bar changes then in a manner depending on its thermal properties and dimensions, and the rate of change thus obtained cannot be exceeded between similar temperature limits. Practical quenching depends on how far these conditions are satisfied, which really resolves itself into the simple question: How constant can the surface of the specimen be kept at the lower temperature after immersion in the liquid, and what properties must the liquid possess to fulfil this purpose best?" McCance accepts Benedicks' conclusion that vaporization plays the most important part in the quenching power of a liquid, and the fact that water with its high latent heat of vaporization is the best quenching liquid known accords with this view.

What happens when a uniformly heated bar is plunged into water at 0° C. is somewhat as follows:—The layers of water in immediate contact with the bar are rapidly heated to their boiling-point and steam is formed; this expands outwards and causes a fresh layer to come in contact with the surface, and so the process goes on. The surface of the bar alternates then between 0° C. and 100° C., the steam acting as a carrier of heat to the body of the liquid; this goes on until the supply of heat is insufficient to form steam, when the transference of heat and the cooling of the bar take place by convection only; and at this stage of the process a high conductivity in the liquid is an actual disadvantage, since it retards convection. A low viscosity is wanted to enable the steam to travel outwards with as little resistance as possible, while a high specific heat ensures that the temperature changes of the liquid are small.

We have next to consider certain fundamental properties of iron and its alloys with carbon which have been established by modern research, and without a knowledge of which any discussion of theories of hardening would be unintelligible. These properties have been for the most part investigated during the last fifty years by a small band of devoted research workers who have been the pioneers in founding the modern science of metallography.

The application of the microscope to the study of the constitution of iron and steel, regarding these materials as igneous rocks, is due to Henry Clifton Sorby, of Sheffield, who died only a few years ago, while the use of the thermo-electric pyrometer as a means of studying the changes of internal energy in the same materials is the work of a Frenchman who is still living—M. Henri le Chatelier. The methods opened up by these pioneers have proved extremely fruitful, and the results achieved by their systematic exploitation have brought certainty into a field where previously only speculation reigned. With these results it is necessary to deal briefly.

It will be convenient first to consider the evidence afforded by accurate pyrometry, although historically the microscope led the way. It is now known that the purest iron which can be obtained, containing as it does not more than three parts of impurity per 10,000, can exist in at least three different modifications, and there is every reason to believe that these modifications are a property of the iron and not in any way due to the small amount of impurities mentioned. If the cooling of a sample of such iron from the melting point (1505° C.) is followed by means of a delicate pyrometer and potentiometer, the cooling is found to be normal for about 600°. At about 900° C., however (Fig. 3), there is a sudden and large evolution of heat which completes itself within a few degrees and is sufficient to arrest the fall of temperature completely for a time; after this the cooling proceeds for an interval, and then another evolution of heat occurs. This is different in character and amount from the previous one; it is markedly smaller in quantity and is spread over a considerable temperature interval, which ranges approximately 780°–740° C. In the latter case the evolution of heat is never sufficient to arrest the fall of temperature completely. Below this point there is no further abnormality in the rate of cooling to the ordinary temperature. These two heat evolutions were rightly regarded by the early workers as marking critical points in the history of the iron ore cooling, and were called by their discoverer, the late M. Osmond, Ar_3 and Ar_2. From the freezing point down to Ar_3 the iron is known as γ, between Ar_3 and Ar_2 as β, and below Ar_2 as α. These three varieties of iron have characteristic properties, of which, from the point of view of the argument of this lecture, the following should be noted: γ iron is completely non-magnetic and has a considerable power of dissolving carbon, the amount of which varies with the temperature; it has also a characteristic crystalline form. β iron is also non-magnetic, but its capacity for dissolving carbon is almost nil, and it also has a characteristic crystalline form. α iron, on the other hand, is magnetic, but in all other respects it resembles β iron very closely, and no difference in crystalline symmetry between these two varieties has yet been discovered. A considerable controversy has raged for many years now as to whether each of these varieties is to be regarded as an allotropic form of the metal. Even now this question is not yet settled, chiefly because there is as yet no consensus of opinion as to the definition of allotropy. If, however, a change in crystalline symmetry be regarded as the criterion of an allotropic change, then γ iron is certainly to be regarded as an allotrope, whereas *β* and *a* iron are to be regarded as the same variety, the only difference being that *β* iron is *a* iron which owing to rise of temperature has lost its magnetic properties. From the point of view of the hardening of steel, the important difference between these varieties of iron is that γ iron can dissolve substantial amounts of carbon, whereas *β* and *a* iron cannot, and the unique position occupied by carbon as a hardening reagent in steel is intimately connected with this difference in properties.

By adding carbon to iron and thus making steel, a marked influence on the critical points of the metal is observed. Taking, for example, a steel with 0·15 per cent

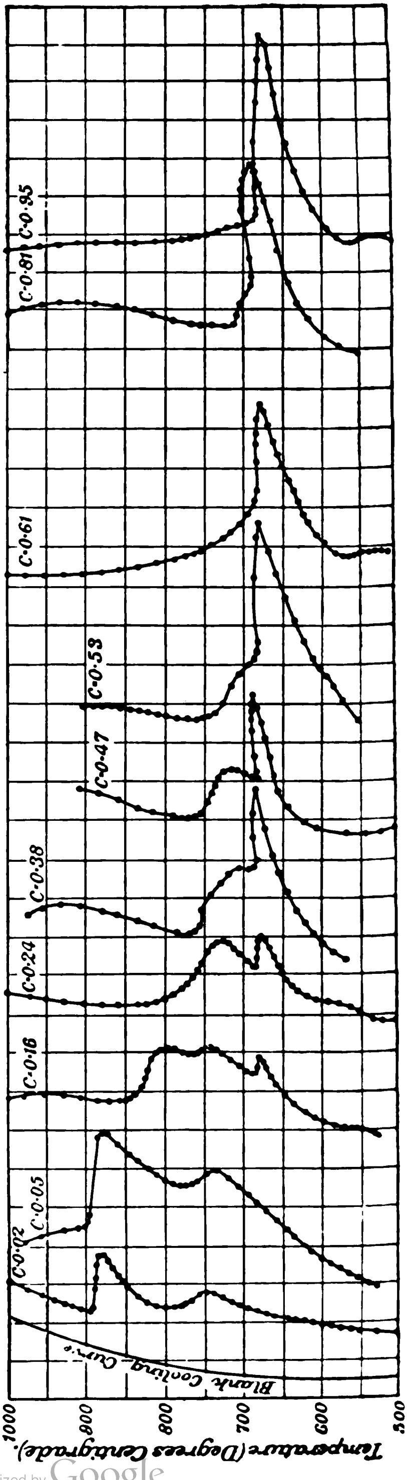

Fig. 3. COOLING CURVES OF IRON AND IRON-CARBON ALLOYS.
DEFLECTIONS OF THE DIFFERENTIAL GALVANOMETER PLOTTED 1/16TH SIZE.

carbon, it is found that the temperature of the Ar_3 charge is lowered, that the Ar_2 charge is unaffected, and that a third critical point makes its appearance at about 700° C., this last-named point being of small magnitude, but completed within a very narrow range of temperature. This is known as Ar_1. Taking next an alloy with 0·35 per cent carbon, the position of Ar_3 is still further lowered, Ar_2 still remains the same, while Ar_1 appears as a much larger point. At about 0·45 per cent carbon, Ar_3 has been so much lowered that it coincides with Ar_2; in other words, at this composition γ iron changes directly to α. Ar_1 is also observed, and has grown in magnitude. With the gradual addition of carbon Ar_3 and Ar_2, which have now become one point, are still further lowered, while Ar_1 increases in magnitude. Finally, at about 0·9 per cent carbon, all three points coincide at one temperature, namely, that of the Ar_1 change about 700° C. It is obvious from the foregoing that while the points Ar_3 and Ar_2 are characteristic of iron, the point Ar_1 is characteristic of iron plus carbon—namely, steel.

The interpretation of the critical points thus briefly outlined has been rendered possible by the combined study of the foregoing method with the microscope, which has revealed the structural changes accompanying these heat evolutions, controlled by a consideration of the requirements of the phase rule, and elaborated into what is known as the Iron-Carbon Equilibrium diagram. This diagram (Fig. 4), whose co-ordinates are temperature and concentration, defines what, at any given temperature and concentration, are the constituents present in an iron-carbon alloy. In so far as knowledge of these is required in connection with the hardening of steel, attention may be confined to the left-hand portion of the diagram between the limits of concentration 0 to 1·5 per cent. The area A G O S E defines the limits of existence of γ iron, from which it will be seen that it can exist from 1500°–700° C., and from 0 up to 1·8 per cent carbon. This constituent is also known as austenite. Within this area the carbon present in all steels is dissolved, and the evidence, both chemical and physical, indicates that it is combined with, as well as dissolved in, the iron as a carbide of iron. Below 700° C. only α iron is stable, and in a normally cooled steel almost the whole of the carbon is precipitated as a crystallized carbide of iron, Fe_3C. At the point S, corresponding to the composition 0·9 per cent carbon, we have the simplest case possible, for the austenite is resolved on cooling below it into a mixture of α iron and iron carbide, Fe_3C. This mixture separates in characteristic alternate plates with a laminated appearance, and is called pearlite from its analogy to mother-of-pearl. It consists of about 86·5 per cent iron and 13·5 per cent Fe_3C, on the assumption that no carbide is dissolved in the iron. This assumption is not strictly speaking true, but the amount dissolved is so small that for practical purposes it may be neglected.

Steels on the left-hand side of S consist, below 700° C., of a mixture of iron and pearlite, whereas those to the right consist of a mixture of iron carbide and pearlite. From this diagram it will be seen why a tool steel containing 0·9 per cent carbon has been chosen for illustrating the theories of hardening. Such a steel when cooled from a high temperature has only the one critical point on cooling, namely, at S, and therefore furnishes the simplest case for discussion.

From a reference to the diagram it will be clear that if a steel tool is quenched from a red heat, say about 800° C., at the moment of quenching the constituent present is austenite. The act of quenching has for its object the rapid passage of the metal through the change point S, and the prevention of the separation of the single constituent austenite into the two constituents of pearlite.

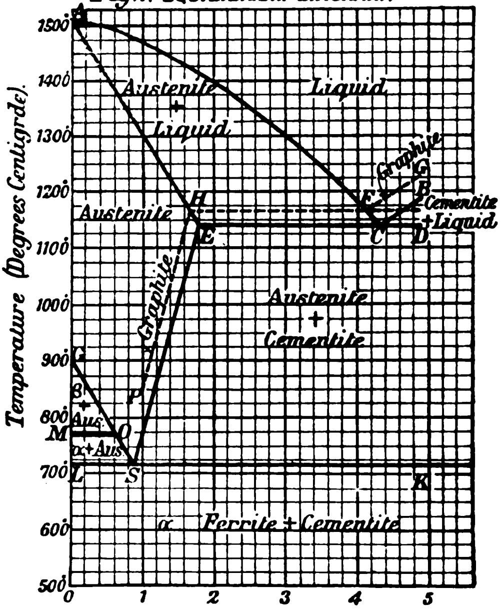

Fig.4. EQUILIBRIUM DIAGRAM.

Coming now to the structures of pure γ iron, γ iron containing dissolved carbon, pure β iron, and pure α iron, the following facts have to be considered:—

Pure carbonless γ iron has never yet been obtained; it is impossible by any quenching, however rapid, to bring down to the ordinary temperature the form of iron stable between A and G. The rate of change is too rapid. By means of either manganese or nickel, however, elements which lower the temperature of this transformation point below the ordinary temperature, it is possible to make alloys containing nothing but γ iron, and these possess a characteristic crystalline form. They consist mainly of polyhedral crystals which exhibit a decided tendency to twin—that is, there are certain planes in each crystal about which the crystal elements tend to rotate through an angle of 180°, thus giving rise to the characteristic twin formation easily recognizable under the microscope. As already mentioned, between β and α iron no difference in crystalline symmetry has been detected. These irons also crystallize in polyhedral crystals, which, however, exhibit no tendency to twin.

According to the equilibrium diagram, the solution of carbon in γ iron known as austenite must crystallize in the form of polyhedral crystals, since it is a solid solution, and these have the same general character as those of a pure metal. The structure of austenite, however, as obtained by quenching within the area of AGOSE, does not correspond to this theoretical deduction. Taking as an example the tool steel containing 0·9 per cent carbon, if this is quenched, say at 750° C., a faintly marked acicular structure is obtained when the specimen is examined. If the temperature of quenching be raised this structure becomes more strongly marked and the needles are decidedly larger, and even at the highest temperatures polyhedral crystals are never obtained. This acicular structure is known as martensite, and it is universally agreed that it is the characteristic structure of properly quenched steel. All the theories of the hardening of steel which have been put forward during the last forty years centre round this one question: What is martensite? If and when this question can be correctly answered it will furnish a complete answer to the question: Why is steel hardened by quenching?

Before considering these theories two other facts must be briefly mentioned. Firstly, the carbide of iron which exists structurally free in an annealed tool steel is a very hard substance, but as it is mixed with soft α iron in the proportion of 13·5 to 86·5, the tool is comparatively soft. This carbide can be separated from the steel by appropriate solvents, which remove the iron. On the other hand, it is not possible to separate any carbide of iron from a properly quenched tool steel. When the latter is treated with dilute acids, the steel dissolves without residue and liberates a complicated mixture of hydrocarbons both liquid and gaseous. There is therefore a fundamental difference in the form of the carbide in a quenched as compared with an annealed steel. In the former it is wholly dissolved, in the latter wholly segregated. When a hardened tool steel is softened by annealing, the carbide of iron, Fe_3C, is gradually precipitated. If the annealing is carried out at low temperatures it is precipitated in the form of ultra-microscopic particles, which are known as troostite. On raising the temperature the troostite passes into another variety known as sorbite, and at still higher temperatures well-segregated pearlite results. The gradual tempering and softening of hardened tool steel is caused by the precipitation of iron carbide in these forms together with soft α iron, and as to this no difference of opinion exists.

Secondly, all tool steels when properly quenched, and thus hardened, are magnetic. As austenite is non-magnetic, and as, so far as is known at present, the only form of iron which is magnetic is α, it follows logically that some α iron is present in hardened tools, and therefore that martensite is austenite which has undergone some change.

Up till recent times there were two chief theories of hardening, which were known as the allotropic and the carbonist. The former was mainly a French, the latter chiefly an English theory. Whatever be the verdict of history on these two theories, there can be no question that each has been justified by the volume and character of the research work to which it has led. The two schools of thought were in many respects sharply opposed, but in the light of the fuller knowledge which exists to-day, it must be admitted that each has made a substantial contribution to scientific knowledge. The allotropic theory is in the main due to the late M. Osmond, who was the first to investigate the critical points of iron and steel by a delicate pyrometer. The history of the development of this theory is particularly interesting, and must be briefly reviewed if a correct perspective of its merits is to be obtained. When M. Osmond first investigated the cooling of pure iron he only detected the point Ar_3; as has been already mentioned, this is accompanied by a large evolution of heat. He called the iron above Ar_3 β, that below it α, and upon this he developed the following argument:—There are two kinds of iron, the atoms of which are respectively arranged in the molecules so as to constitute hard and soft iron, quite apart from the presence or absence of carbon. In red-hot iron the mass may be soft, but the molecules are hard; let this be called β iron. Cool such red-hot pure iron, whether quickly or slowly, and it becomes soft; it passes to the α modification because there is nothing to prevent its doing so. If, however, carbon is present and the steel be rapidly chilled, the following result is obtained: a certain proportion of the molecules are retained in the form in which they existed at a high temperature—namely, the β modification, which is hard—and hard steel is the result. According to this view, then, the hardness of quenched steel is due directly to iron and only indirectly to carbon.

Subsequently, when M. Osmond had perfected a more refined method of taking cooling curves he discovered the critical point at

Ar_2. As already mentioned, the evolution of heat is very much less than at Ar_3, and for some time he was unable to decide whether Ar_2 was merely the termination of Ar_3 spread over more than 100° or was an independent point. Finally, when he had shown that at Ar_2 magnetic properties appear in the iron, he decided that this point was independent of Ar_3. The logical consequence of this was that three varieties of iron had to be considered. Now comes the important point—M. Osmond changed his theory of hardening, and put forward the view that it was not the iron above Ar_3 which was hard, but the variety which existed between Ar_3 and Ar_2. The iron above Ar_3 was called γ; that between Ar_3 and Ar_2, β; and below Ar_2, α.

The difference between the two theories will now be obvious. Whereas in the former the hard form of iron was that which existed in what we know to be the γ region, in the latter it was the variety which existed in the narrow temperature range between Ar_3 and Ar_2. Hardening was thus due to an intermediate form of iron between the varieties stable at high and low temperatures. According to the new theory β iron was still supposed to be the cause of hardness, but the new β was quite different from the old one, and progress in the development of the theories of hardening would have been more rapid if it had been generally realized that the original hard β iron was, according to the more accurate knowledge revealed by later work, γ iron. The new theory presupposed the existence of all three forms of iron in a quenched tool, and according to it the act of quenching permitted the retention of only a portion of the γ iron, while the remainder was converted into a mixture of β and α, the former conferring the hardness, and the latter the toughness and magnetic properties.

This theory was enthusiastically adopted by many workers, though certain modifications of it were put forward from time to time. Whereas the Osmond theory predicated that the β iron separated in a carbonless form, the whole of the carbon being retained in the γ iron, one of the modifications put forward was that the β iron contained some dissolved carbon. The theories, however, all had this in common, that the hardness of quenched tool steel was supposed to be due to a crystalline variety of iron, namely, β, intermediate between γ and α. According to these theories, then, the special constituent present in quenched steel, namely, martensite, is due to β iron.

It is possible to test this theory to-day by fuller knowledge, and it will be sufficient to advance two considerations which seriously invalidate it. In the first place, if the equilibrium diagram be examined, it will be seen that in the case of all tool steels—that is, those containing from 0·7–1·5 per cent carbon—γ iron passes directly to α and not by way of β iron at all, and it is difficult, though not impossible, to see why β iron should be formed. The usual answer made to this objection is that the diagram refers only to the stable varieties of steel, and that during quenching fugitive and unstable varieties are formed, one of which may be β iron. I cannot help thinking that this answer would never have been thought of if the equilibrium diagram had been well established before M. Osmond advanced his theory. The fact is, however, that when the allotropic theory was put forward knowledge of the diagram was very far from being as complete as it is to-day.

In the second place, this theory can be tested by experimental work published nearly six years ago by Messrs. Rosenhain and Humfrey.* These investigators carried out a research on the tenacity of mild steel containing 0·1 per cent carbon between 1080° and 612° C. This material contained therefore α, β and γ iron. Strictly speaking, their research does not test hardness in the ordinarily accepted sense of the term, but it cannot be denied that determinations of tenacity do test the cohesion of a material which may be properly regarded as intimately connected with hardness. The authors found what they describe as two very well marked discontinuities in the temperature—tenacity curve, corresponding to the critical points A_3 and A_2; but whereas the discontinuity at A_3 was large that at A_2 was quite small, and their results must be regarded as negativing the view that β iron is harder than γ iron. In fact, if their curve be examined it will be found that the iron has a decidedly greater tenacity just above Ar_3 than just below it, even though a decrease in tenacity would be expected owing to the higher temperature. At 950° C., where their iron was in the γ range, the tenacity was markedly greater than at 850° C., where it was in the β range. It is very interesting that these results of Rosenhain and Humfrey fit in better with the original theory of Osmond, which was really a γ iron theory, than with the later one.

It must also be regarded as improbable that Beta and Alpha iron, which, apart from the fact that the former is non-magnetic and the latter magnetic, only exhibit slight differences in properties, and, in particular, between whose crystalline forms no difference has yet been discovered, should differ so strikingly in one particular property—hardness—such as is demanded by the above theory. From the work of Sir George Beilby, which will be subsequently quoted, it would appear that two varieties of a metal possessing the same crystalline form would be likely to have the same hardness.

The carbonist theory of hardening, as the name implies, throws the chief, in fact the sole, emphasis on the function of carbon. It owes its inception to the pioneering work of Sorby, of Sheffield, who made a very careful study of the micro-structures of quenched and annealed steels. On this view, the hardness of quenched steel is to be ascribed to the conversion of the pearlite, stable below Ar_1, into an amorphous constituent called hardenite, which corresponds empirically to the formula $Fe_{24}C$. According to the views of later carbonists this hardness is the property of this solid solution, which is stated to be amorphous, and the whole of the carbon is regarded as having been trapped by quenching and retained in solid solution.

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* Journal of the Iron and Steel Institute, No. 1, 1913, pp. 219–268.

Modern research has shown that this theory is right in its assumption that the whole of the carbon is dissolved in a properly quenched steel, but in their desire to disprove the allotropic theory of hardening the exponents of this theory have gone to the unwise extreme of denying that the allotropic forms of iron have any influence on the hardness of tool steel at all. When it is remembered that iron carbide is practically insoluble in α or β iron, but is only soluble in γ iron, it is certainly unscientific to deny that γ iron has any share in the hardening process. Moreover, the theory as stated in the above form is both incomplete and contradicted by certainly one fact. It is certainly not correct to say that the constituent formed on quenching a tool steel above A_1 is amorphous, if by that is meant that it is wholly amorphous; the structure of a quenched tool steel is in the main crystalline, although, as will be subsequently suggested, some amorphous material may be present. The carbonist theory does not explain the martensitic structure of a quenched tool. If there was nothing more in the hardening process than is contained in the carbonist theory, the structure of a quenched steel should consist of polyhedral crystals, because this is the typical structure of a solid solution. Further, the hardness of quenched steel is much greater than that which can be explained merely by the retention of iron carbide in iron. Some other factor must enter in. Finally, the fact established by McCance, that on adding carbon to iron the hardness of the quenched steel increases up to 0·7 per cent carbon, and remains constant between this point and 1·18 per cent, receives no explanation from the carbonist theory, and appears to point to the conclusion which McCance has already drawn, that at any rate some of the hardening must be due to the iron itself, and that there is a definite limit to the amount of hardening that iron can stand.

These two theories mainly held the field for the best part of twenty years, and anyone who reads the history of this stimulating period in the development of metallographical thought cannot, I think, avoid the conclusion that the allotropists made a serious mistake in laying such exclusive emphasis on the function of iron, while the carbonists made an equally serious mistake by denying that iron had anything to do with hardening at all.

I pass now to consider some important research work, which, although it was not carried out on iron or steel or indeed any other alloy of iron, has nevertheless profoundly influenced the views of metallographers with regard to the causes of the hardening of steel by quenching. This is but one instance of many which the history of scientific discovery provides, that the great advances in any particular field of knowledge are not as a rule due to workers in that field, but to those whose labours apparently have little direct relation to them. In this case I refer to the researches of Sir George Beilby on the hard and soft states in metals. Those researches were carried out on metals which could be obtained in a very high degree of purity, and which did not possess, so far as is known, any critical points, such as those that characterize the metal iron.

The experiments were carried out for the most part with gold, silver and copper, and there is no doubt that he made a wise choice in selecting these particular materials, which are most suitable for the particular study which he had in view. His early researches were concerned with the microscopic study of the structure and behaviour of thin films of metal, and this led him to the discovery of the nature of the operation of polishing, etc. He found that in this operation a true skin is formed over the polished surface, and that this gives unmistakable signs that it has passed through a state in which it must have possessed the perfect mobility of a liquid, although the operations were carried out at the ordinary temperature, which is several hundred degrees below the melting-points of the metals in question.

The metal itself in the annealed condition is composed of crystals, but the skin formed in grinding and polishing possesses distinctive qualities which differentiate its substance very clearly from that of the unaltered substance beneath it. It is for instance much harder, and even when formed on the face of a crystal on which the hardness varies in different directions, its hardness is the same in all directions. Since the skin can be dissolved off in stages, it was possible thus to gain an insight into the history of its formation. The surface skin can thus be investigated analytically as well as synthetically; it can be built up in stages by polishing, and it can equally easily be removed in stages; the latter operation having the advantage over the former that the method of solution lends itself to a step by step removal of great refinement and accuracy. The discovery that layers of a solid many hundreds of molecules in thickness can have the mobility of the liquid state conferred upon them by purely mechanical movement, opened up a new field of inquiry into the internal structure of metals which have been hardened by cold working. As a result of this inquiry, Sir George Beilby put forward a theory of the hard and soft states of metals, according to which hardening results from the formation at all the internal surfaces of slip or shear of mobile layers similar to those produced on the outer surface by polishing. These layers only retain their mobility for a very brief period, and then solidify in a vitreous amorphous state, thus forming a cementing material at all surfaces of slip or shear throughout the mass. The keystone of the theory is thus the instant liquefaction followed by the solidification of sheets of molecules during mechanical deformation.

It was never found possible, even by the severest deformation, to convert a crystalline into a completely amorphous metal. The resulting product was always a complex of the two varieties. In using the expression "amorphous," Sir George Beilby was careful to explain that amorphous meant non-crystalline, in the sense that the molecules are not marshalled in crystalline order and orientation, while the addition of the qualifying term vitreous narrowed the field to substances which in some degree resemble the glass-like form assumed by the silicates on cooling from the molten state. Glass is a typical amorphous substance, the useful qualities of which depend entirely on the fact that it is in the non-crystalline state. As is well known, it is a very hard substance.

By careful experiments Sir George Beilby was able to show that the amorphous constituent formed by cold work is not thermally stable, but that on annealing it reverts at a given temperature to the crystalline state. Well-marked differences of properties, according to whether the metal was wholly crystalline or a complex of vitreous-amorphous and crystalline units, were established. Thus, the crystalline phase of a given metal was shown to be thermally stable but mechanically unstable, whereas the amorphous phase was mechanically stable and thermally unstable.

Subsequently, in the discussion on a paper by Professor Edwards and myself,* Sir George Beilby explained why in his view the vitreous-amorphous was harder than the crystalline phase. By hardness was meant that the metal was more or less rigid, and that it had the power to resist larger or smaller deforming stresses without suffering permanent deformation. He proceeded to consider how this rigidity might be affected by the molecular structure of the metal. "One fundamental property of individual molecules was that they were space-filling entities which resisted the encroachment into their domain of other molecules; their bounding surfaces were probably kinetic, but they were none the less sharply defined. Another fundamental property of molecules was that they attracted each other strongly; that mutual attraction gave rise to cohesion, the quality in virtue of which the physical would continue to hold together and exist. In a mass of metal or in any other solid aggregate, there was a certain available amount of cohesion which was the product of the specific cohesion of each molecule and the number of molecules present; the rigidity of any aggregate would be determined by the use to which the available cohesion was put in the actual structure. Taking the simplest of all possible forms for the molecular unit, the sphere, it was known that there were various ways in which the spheres might be built up into a homogeneous structure. In the most densely packed form, each sphere within the mass was in contact with twelve others; in that form there was the condition of greatest rigidity, the molecular centres of attraction being as close to each other as was possible in any unstrained structure. But close packing of that form was not by any means common in crystal structures, indeed it was not quite certain that it existed at all ; it certainly did not by any means represent the inevitable completion of crystalline effort ; in the crystal, therefore, the structure was not necessarily or even probably the most rigid possible structure. The available supply of internal cohesion was not all used up to secure maximum rigidity. In the less closely packed assemblages of spheres it could be imagined that by a small translation of the layers of molecules relatively to each other, a more rigid structure might be produced. If, for example, in an assemblage of spheres in which each was only in contact with six others, a displacement occurred which brought each into contact with twelve others, it was clear that the new structure would be at least twice as rigid as the original structure. . . . It was believed that in the metals which crystallized in the regular system, such as gold, silver, copper, iron, etc., the molecules were not in closest packing; in these metals the crystalline state was one of great mechanical instability and consequently of very small rigidity ; as a result of that, disturbances by deformation caused very widespread breaking down of the crystal structure with momentary liquefaction of molecular layers and groups. If that occurred at a point below the crystallization temperature, the newly solidified portions could not receive their crystalline arrangement, and remained chilled or frozen into a more rigid type of structure, a type in which the available cohesion of the mass was more fully utilized than in the crystalline state. . . . In the case of ductile metals, flow under deforming stresses followed by re-solidification left a proportion of the molecules in a condition of constraint, their freedom as vibrating units being impaired. . . . If then the available cohesion of the mass could in that way be so effectively utilized that it could put a constraint on the vibration of the individual molecules, it was not at all surprising that the molecules were so much more closely held by each other, that a greater force was then required to move them over each other under deforming stresses."

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* Journal of the Iron and Steel Institute, No. 1, 1914, pp. 178–180.

In the light of investigations such as the foregoing, the hardening of steel by quenching can be viewed from a broader standpoint than was possible to earlier researchers. We know to-day that metals may be hardened in four distinct ways : (1) pure ductile metals can be hardened by cold work ; (2) these metals may be hardened by being alloyed with one another, or with certain non-metallic elements such as hydrogen, nitrogen, carbon, etc. ; (3) if these mixtures are ductile, they may be further hardened by cold working, for instance *a* brass (70 copper, 30 zinc) ; (4) certain of these alloys can be hardened by chilling. The hardening of steel by quenching comes in this class. It is important to remember that the rapid drop in temperature which quenching produces may act in two ways :—(*a*) "It may stereotype the form of chemical combination or of structure, crystalline or non-crystalline, which is in the condition of equilibrium at the temperature at which chilling begins. (*b*) It may set up contraction strains within the cooled mass; these strains may be wholly or in part either within or beyond the elastic limits. When the strains exceed the elastic limit, and permanent deformation occurs, then hardening may result as in (3)." *

One other fact must still be mentioned. Charpy and Grenet have shown that in iron containing very small amounts of carbon, an expansion occurs on passing the Ar_3 point—that is, from γ to β iron; and further that in steels of tool-steel composition the separation of iron carbide from austenite on cooling is also associated with an expansion.

I pass now to consider three of the most modern theories of hardening; two of these were enunciated simultaneously and independently in May, 1914, the third in November of the same year.

The first of these is due to Dr. McCance. As already mentioned, he showed that when the carbon reaches about 0·7 per cent a maximum hardness is obtained in the quenched state, and this value is not exceeded in any pure carbon steel. He reasons that if the hardness were the direct consequence of carbon in solution it would be proportional to the amount of carbon dissolved; that it is not so shows that the action of carbon is indirect, and that the hardening element is the iron itself. In the annealed condition the ball hardness of this steel is 174, while in the hardened condition it is 713.

Holding as he does the view that β iron is not a definite allotrope of the metal, but merely α iron which is non-magnetic from purely thermal causes, McCance considers that at Ar_3 γ iron passes directly to α. He explains the liberation of heat at A_2 as being due to the rapid change in specific heat which necessarily accompanies the ferromagnetic transition. He points out that since α iron loses its magnetic properties above a certain temperature, it might be suggested that γ iron, which at higher temperatures is non-magnetic, might itself become magnetic at lower temperatures. Hadfield has shown, however, that non-magnetic manganese steel (γ iron) is not transformed even at the temperature of liquid air; and McCance reasons that as no treatment can make austenitic steel magnetic which does not increase the specific volume at the same time, this points conclusively to its transformation to α iron, since the thermal magnetic transition makes no appreciable alteration in the specific volume His theory of hardening is based on the following considerations and reasoning:—Since steels, which at the temperature of quenching are non-magnetic, are magnetic in the hardened state, this change must have taken place during the time taken by the quenching; therefore some of the original γ at any rate must have changed to the magnetic α condition. Measurements of the electrical resistance, however, show that the carbon still remains in solution; therefore under the conditions of quenching the transformation of the iron from the γ to the α state can take place independently of the change in the state of the carbon from one of solution to that of precipitation. Thus, these two transformations, which on slow heating and cooling take place simultaneously, can behave as independent reactions, although in a limited sense. "For, though martensite may be considered as an enforced solution of carbon in α iron, it must not be forgotten that there is still some γ iron present with the carbon, which is absolutely necessary to maintain its solubility, and any change in the state of the carbon causes this transformation of γ iron to α. It might be on this account be suggested that the iron is chemically combined with the carbon, but an examination of the curve connecting the loss of magnetic saturation intensity with the carbon content shows that the γ iron increases much more rapidly than the amount of carbon, which points to purely physical influences." * McCance then considers the cooling of a hypo-eutectoid steel, that is, one containing rather less than 0·9 per cent carbon. As he points out, its complete transformation into α iron and cementite involves two distinct transformations: (*a*) a change in the state of the iron from γ to α; (*b*) a change in the state of the carbon. The first of these can take place independently of the second, but when the second takes place it necessarily involves the first. There are therefore three possible conditions:—

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* Transactions of the Faraday Society, Beilby, November, 1914.

1. Both transformations are completed, producing pearlitic steel which is soft.

2. Both transformations are suppressed. This has never been achieved with any pure iron carbon alloy, and the addition of a certain amount of manganese is necessary, as Maurer has shown. In this case pure austenite results, and this, while harder than the steel in the pearlitic condition, is nevertheless softer than a hardened steel.

3. When (*a*) takes place, but (*b*) is suppressed, then the resulting structure is martensitic, and, according to McCance's theory, the α iron formed under this condition is interstrained and very hard. Case 3 then is the important one from the hardening standpoint, and to be possible it is necessary that a difference should exist in the respective velocities of transformation of (*a*) and (*b*), so that with an appropriate rate of cooling the slower transformation can be suppressed, while the faster still takes place. On McCance's view then, it is interstrained α iron which is the cause of the hardening of steel. He does not, however, accept Beilby's theory of a hard vitreous amorphous phase. His view of interstrain is as follows: "Metals in their normal unstrained condition are crystalline, that is, they are composed of an orderly arrangement of their constituent atoms arranged in a space-lattice which conforms with whatever symmetry the crystals possess. Successful attempts have been made by the aid of X-rays to determine the actual position occupied by the atoms.

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* Transactions of the Faraday Society, 1914, p. 56.

and copper, for instance, has been shown to have its atoms built up in a 'face-centred' cube, that is, a cube having an atom at each corner, and one at the centre of each face. The effect of deformation on such systems must be to alter the position of each atom relatively to its neighbour, and permanent deformation must cause so much alteration that the atoms along the planes of slip can no longer return to their positions of equilibrium. This corresponds to the condition of interstrain and results in a hardening of the material." * McCance admits that the destruction of the crystalline arrangement does go so far as to render the material in these planes of greatest movement actually amorphous, but appears to deny that this amorphous phase is hard.

The important connection between rate of cooling and hardening is well illustrated by the behaviour of alloy steels. If, for instance, manganese is added to the steel it lowers the temperature of the transformations and greatly decreases their velocities, so that with sufficient manganese even at ordinary rates of cooling the structure is martensitic, and the steel very hard and brittle. The addition of even more manganese ultimately prevents any transformation taking place, even when the steel has cooled to the ordinary temperature, and a pure austenitic steel is the result. If the martensitic manganese steel be water-quenched, not only is it not hardened but is actually softened, this being due to the complete suppression of both transformations and resulting in the production of an austenitic steel. Similar results may also be obtained with nickel.

The following experiments quoted by McCance are also important as bearing out his contention that the main cause of the hardening of steel lies in a change in the iron itself. A steel containing 3·6 per cent nickel and 0·19 per cent carbon had a normal hardness of 183, which on quenching in water from 1000° C. had risen to 444, whereas a steel containing the same amount of carbon but no nickel increased after the same treatment from 143 to 218. In other words, whereas the carbon steel increased only 65 points, the nickel steel was augmented by 259. As nickel does not form a carbide in this range of composition, the state of the carbon is similar in both alloys, consequently the increased hardness must be due to the influence of the nickel on the iron, and McCance explains this by the view that the iron is enabled to retain a greater degree of interstrain by lowering the temperature at which the transformation of γ to α iron takes place. He rightly lays stress on the importance of internal friction in retarding transformations and preserving metastable states.

McCance's theory of hardening has been built up by acute reasoning on a solid foundation of well-established facts. As yet, however, it cannot be regarded as constituting a *complete* theory, because it is intentionally non-committal with regard to the expression "interstrain." As he points out, the theory of interstrain action is in itself an inquiry, and that inquiry has yet to be undertaken.

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* Transactions of the Faraday Society, 1914, p. 53.

The second of the most modern theories of hardening was put forward by Professor Edwards and myself.* We started from the position that steels are not the only alloys which can be hardened by quenching in water from moderately high temperature. Many alloys, in fact, are known which possess this property. The difference is one of degree and not of kind. Steels, in fact, are simply the higher members of a group of different alloys which possess the same characteristics in varying degrees. Further, all the alloys which behave in this way have similar constitutions at high temperatures, and undergo precisely similar changes as they cool to atmospheric temperature. They undergo decomposition at certain critical temperatures which are marked by evolutions of heat. It is generally admitted that the hardening produced by quenching any of these alloys is in some way related to the effect of quick cooling upon these critical heat changes. Within certain limits the effect of varying the rates of cooling upon the Ar_1 change in steels and other similar changes is well known. As the rate of cooling is increased, the temperature at which the changes occur is progressively lowered, and simultaneously the thermal magnitude is decreased. It is therefore in agreement with established facts that, as the rate of cooling is increased, there is an increased tendency to prevent the critical change taking place, and to keep the heat of the change suppressed. The careful experiments of Professor Benedicks show that when the highest quenching velocities are attained there are no indications of any heat at all being evolved. Logically, therefore, these facts appear to point to the view that the hardening is connected with the suppression of the heat of the inversion. If this be the case, energy has to be brought to bear upon the specimen during the quenching process in order to overcome the tendency for the inversion to take place. This energy is considered to be brought to bear in two ways :—

1. By the sudden contraction of the outer shell or envelope of the specimen.
2. By an internal molecular contraction of the mass which is connected with the solution or osmotic pressure of the dissolved carbon in the case of steel.

This explanation, so far as it goes, is a perfectly general one, applicable to all alloys which may be hardened by quenching.

The next step in the argument is as follows :—In the case of all alloys hardened in this way when the heat change is suppressed, severe internal stresses are set up which exceed the elastic limit, and consequently cause an internal straining of the material. That, in the case of steels, the metal is internally strained appears to us well established. According to the view put forward by us in 1914, martensite is simply twinned austenite produced by this internal movement, and further along the twinning planes amorphous material is produced by the distortion of the crystalline units, which material we regard as corresponding to Beilby's hard vitreous-amorphous phase produced by mechanical work. In order to establish this theory therefore evidence should be produced that these actions take place. For such evidence in full the original paper must be consulted, and only the briefest résumé can be given here. The view that internal straining occurs during the quenching of steel has only been challenged by one authority, Dr. Rosenhain, who says: "This view meets with the insuperable difficulty that although quenching does set up severe stresses in steels, it does not cause any serious flow or movement; the strain hardening of metals only becomes marked when severe plastic flow has occurred." This objection however breaks down on examination. It is true that in cold working a metal a large degree of strain is needed to produce great hardness, but in this case the direction of strain is constant throughout the mass, so that the strains are additive, and these result in a great change of outer dimensions. But the strains in the slipping planes set up by hardening from quenching are quite different. They are not in constant but in opposite directions along different internal planes, and they therefore offset each other and do not deform the mass as a whole. That severe straining does occur therefore may be accepted as established. The microscopic evidence for this straining was regarded by us in our original paper as being due to a great development of twins in the steel. It was pointed out, however, in the discussion that although twinning might have occurred, this would not necessarily cause any increase in hardness, since the two halves of every twin should dovetail into each other, and therefore amorphous material need not necessarily be produced in the twinning planes. This may be so, but for the purpose of this theory it does not matter whether the movement is regarded as taking place along twinning or along slip planes; in the latter case it is generally agreed that amorphous material must result by the distortion of the crystalline units. According to this view, then, martensite is austenite which has been distorted by the formation of amorphous films along the slip planes of γ iron. It will be observed that I have already begun to use Sir George Beilby's theory of the hard amorphous state in order to explain the hardening of steel by quenching, by suggesting that amorphous material is formed along the slip planes of γ iron. What is now required is to show, if possible, that these layers persist on quick cooling. Beilby has shown that when strain-hardened metals are heated, the amorphous layers re-crystallize, and the actual temperature at which this occurs varies with the particular metal and with the degree of strain hardening. Accordingly, if in quenching the rate of cooling is sufficiently quick, the temperature of the mass will be lowered below that at which recrystallization occurs. In the case of pure iron it has been shown by Chappell and Goerens that the minimum temperature for the strain hardening of iron is about 500° C. It is quite certain that the minimum temperature for a steel containing 0·9 per cent carbon will be higher than this. How much is not known, but let the figure 500° C. stand for this particular steel. In hardening the steel is quenched from a temperature which should not be higher than 800° C. The important interval therefore in the quenching operation is between 800° C. and 500° C. In one of Benedicks' experiments, to which reference has been already made, the time taken to pass through this interval was exactly one second; in the case of a tool of about one inch diameter it will take longer for the centre to pass through this interval, but the probability is that it will not take more than two to three seconds for the skin of the tool to pass through this interval. In such a case, therefore, our view is that the vitreous amorphous layers formed will certainly not have time to re-crystallize, but will be preserved, and hardening will result. It would follow, then, that the final cause of hardening by quenching is exactly the same as that of hardening by cold working. In the case of tool steels and other alloys of a similar nature, the actual stresses which are set up in quenching, which give rise to the twinning or slipping, are brought into action only in the very brief period during which the mass is cooling; therefore, while the metal is undergoing inter-crystalline slip and the amorphous layers are being formed, the chances for these to re-crystallize become less and less as the cooling proceeds. Indeed it may be that a large proportion of the slipping which occurs takes place at moderately low temperatures below that at which the amorphous layers are able to re-crystallize. Put briefly, then, our theory of the hardening of steel is, that it is due to the formation of hard vitreous-amorphous films of a solution of carbide in γ iron formed along the slipping planes of the austenite.

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* Journal of the Iron and Steel Institute, No. 1, 1914, pp. 138–177.

So far as I am aware, there is only one property of quenched steels which is not accounted for by this theory, namely, that all quenched steels are magnetic. It has not been shown that amorphous γ iron is not magnetic, though it is not at all probable, since the property of magnetism is almost certainly associated with a definite orientation of the molecules in iron and steel. Admitting, as may be done on present evidence, that ferro-magnetism is a property of α iron, our theory can be brought into harmony with the fact that all quenched steels are magnetic, by adopting McCance's view that some γ iron passes to α in the quenching of all steels.

The third modern theory of hardening to which I wish to allude was put forward some six months after the two theories which have been briefly outlined. It is due to Mr. J. C. Humfrey.* This theory rests upon the assumption that a hard amorphous phase is responsible for the hardness of quenched steel, and in this respect resembles the theory put forward by Professor Edwards and myself. The formation of this phase, however, is attributed to entirely different considerations, which will now be briefly set forth. Mr. Humfrey considers in the first place what happens when one allotropic form of an element passes into another. He starts from the theory of crystal structure already alluded to in McCance's theory, namely, that the centres of gravity of the molecules are arranged together according to one of a series of geometrical devices, called space-lattices. In each molecule of such a crystal the atoms are similarly situated.

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* **Transactions of the Faraday Society, 1914, pp. 35–43.**

When an allotropic change occurs in such a crystalline body, there is an internal re-arrangement of the atoms in each molecule, and if the new form which the molecules take involves a corresponding change in the external forces which they exert upon one another, the space-lattice may become unstable and a different one formed. Before, however, the reorganization can be completed, it is assumed that there must be temporarily a state of disorder, and it is during this period that Humfrey considers that the structure must be considered as amorphous. This intermediate state is regarded as corresponding to the liquid "which would be formed by the fusion of the solid phase stable at the lower temperatures, if the conditions could be so adjusted that the subsequent re-crystallization were avoided." The temperature-tenacity curves obtained by Rosenhain and Humfrey are regarded as indicating that the melting points of α and β iron would be considerably lower than that of γ iron, if they could be preserved unchanged up to their melting points.

In the next place consideration is given to the fact that physical changes of state do not take place instantaneously throughout the whole mass of any given substance, but that they start from certain nuclei and proceed outwards. From this Humfrey conceives that the intermediate amorphous phase existing during an allotropic phase begins to re-crystallize almost as soon as it is formed, "and is present at any particular instant during the change, merely as thin films or layers between the co-existing crystalline phases, the films moving forward as the change progresses. The course of the breakdown of the one crystalline phase to the amorphous would tend to follow those planes in which the freedom of movement was greatest, namely, the crystal-gliding planes and possibly the intercrystalline boundaries."

With regard to the formation of a new crystalline phase Humfrey points out that the temperature at which it occurs must not be too low for the forces of crystallization to overcome the viscosity of the amorphous phase. If the viscosity is sufficient to prevent recrystallization, the amorphous phase formed will be practically stable. Even in cases, however, where an allotropic change normally takes place at a temperature well above that at which the viscosity is sufficient to prevent recrystallization, abnormal conditions such as rapid cooling may delay the change to well below this temperature. He then applies these general considerations to the case of steel. γ iron normally inverts to β iron at about 900° C. The recrystallization of work-hardened α iron is very slow below 500° C. This gives an interval of 400°. In the case of the 0·9 carbon steel, however, this range is halved, since γ iron does not invert till about 700° C. on cooling. From this it is argued that with increasing carbon contents there is more likelihood of some of the amorphous phase formed by the breakdown of the γ constituent being retained than in pure iron itself. Another factor also enters in which helps to promote the retention of the amorphous phase. Not only the iron, but also the iron carbide has to be considered. When the austenite breaks down, the carbide molecules are regarded as remaining "closely intermingled with those of the α iron." Before the amorphous material can recrystallize two different kinds of molecules have to segregate, those of iron and its carbide; this segregation must be a slow process in an undercooled viscous mass, and rapid cooling is regarded as allowing the minimum temperature of crystallization to be passed before it has had time to take place.

Benedicks has shown that pressure aids the retention of the austenitic condition in steels. The change from the crystalline to the amorphous state in metals is in practically all cases accompanied by an increase in volume; γ iron is denser than α iron, and therefore even denser than amorphous α iron. Accordingly Humfrey argues that after the formation of a certain amount of amorphous material, the pressure set up will be sufficient to retain some of the austenite in the unchanged condition. According to him, then, the particular structure known as martensite is due to two constituents, (1) γ iron (austenite); (2) "an amorphous solution of α iron and carbide": the form in which the two constituents occur being due to the tendency of the austenite crystals to break up along their gliding planes. It will be seen therefore that the conclusion reached by Humfrey is almost identical with that arrived at by Professor Edwards and myself, but that the actual mechanism of the change is different.

As to whether on passing from one allotropic form to another it is necessary to assume the existence of an intermediate amorphous state as postulated by Humfrey, it is not necessary for my present purpose to discuss. It may be pointed out, however, that the passage from the crystalline to the amorphous condition involves the absorption of energy, and he has not explained how that energy can be supplied from the interior of a cooling mass of material, and to render the theory complete in this respect an explanation must be forthcoming. On the other hand, when, as Professor Edwards and I have supposed, both slipping and perhaps twinning of the austenite crystals occur as a result of the quenching stresses, energy is absorbed and may be stored up in the amorphous layers.

Within the limits of this lecture it has not been possible to consider anything like the whole of the evidence which may be brought to bear on the problem of the cause of the hardening of steel by quenching. In particular, that which is furnished by the behaviour of alloy steels, specially those containing nickel or manganese, or both, has enabled the conditions intermediate between austenite on the one hand, and pearlite on the other, to be studied in a more systematic and comprehensive way than is possible with pure carbon steels. Moreover, the discovery and widespread use of the so-called high-speed cutting tools, all of them alloy steels, has widened the scope of the problem to an extent which could not have been realized as recently as twenty years ago. It will be observed that the three modern theories, of which some account has been given, present considerable resemblance to one another, in spite of certain differences which are obvious on the surface. The theories of Humfrey and Edwards and myself attribute hardening to the existence of a hard amorphous constituent in the steel. McCance does not go so far as this, but regards interstrain as the cause of hardness, a condition which I think it is fair to describe as occupying an intermediate position between crystalline and amorphous materials. Perhaps at no very distant date McCance will develop a theory of interstrain action.

The acute differences which divided sharply the allotropists and the carbonists find no counterpart in the schools of thought of to-day. Indeed it is interesting to reflect for one moment on the position of the allotropic theory of Osmond, in the light of the most modern views on the subject. According to these, M. Osmond was correct in attributing hardness to something—stated in its most general terms—intermediate between crystalline γ and crystalline α iron. The view that this intermediate something is a crystalline β iron such as he supposed has not been completely abandoned, but is not held by many workers to-day. This conception has given place to the interstrained iron of McCance and the amorphous iron and iron-carbide complex of Humfrey, Edwards, and myself. The important influence exerted by carbon, insisted upon by the carbonists, is fully recognized in the modern theories described. Speaking with the ampler knowledge of to-day one may summarize the position by saying that the older theories, tested and to some extent corrected by this, have contributed to a broader view of the matter, in which their apparent differences have been put into their proper proportion, and the elements of truth they contained incorporated into other theories which, for the time being, are more in accord with such knowledge. That these in their turn are likely to be modified and amplified by the advance of knowledge I should be the first to admit.

[H. C. H. C.]

WEEKLY EVENING MEETING,

Friday, March 14, 1919.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Professor Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S. M.R.I.,
Fullerian Professor of Physiology.

The Organ of Hearing from a New Point of View.

According to the theory put forward by von Helmholtz in 1863, the internal ear may be regarded as a microscopic piano, furnished with resonating strings, almost ultra-microscopic in size, and some 16,000 in number. Each string or set of strings is supposed to pass into a state of vibration when its sympathetic note enters the ear. Each string or set of strings is supposed to have a corresponding nerve-fibre, which is stimulated by the vibrations and carries them towards the brain as nerve messages. We must also suppose that these nerve-fibres lead ultimately to a central nerve-cell station or exchange, where 16,000 nerve-cells receive messages from their corresponding ear strings. The sorting out and recognition of the messages streaming on from the various strings must fall, even if Helmholtz's theory is accepted, on central nerve exchanges. However satisfactory from the point of view of the pure physicist, Helmholtz's theory of the ear from the point of view of the psychologist, physiologist, or the anatomist, is an impossibility. The strings are present in the internal ear, but they are so placed and so conditioned that the one thing they cannot do is to vibrate; Nature has taken the utmost care to render individual vibration an impossibility. In a theory first put forward by Sir Thomas Wrightson in 1876, and recently elaborated in his treatise on the "Mechanism of the Internal Ear" (1918), the cochlea is supposed to act as a single machine; he has shown it to be the most minute and most delicately adjusted spring balance ever evolved or invented—one designed not only to weigh the simplest and slightest sound wave but also the most complex and voluminous. The ear not only weighs every fluctuation in pressure but automatically registers and records the minutest variation, and through the hair-cells or semaphores which form an intrinsic part of the machine the system of messages or semaphoric signals transmitted from the ear may be compared to the dot-and-dash system of the Morse code. The whole of the organ of Corti is involved in the production of this code of signals; all the fibres of the auditory nerve are concerned in its transmission from the ear to the brain. It is a legitimate inference to suppose that the time signals carried by this code can be deciphered and sorted out at nerve synapses in the central nervous system. Thus Sir Thomas Wrightson's theory brings hearing into line with smell, taste, sight and touch, whereas Helmholtz's theory, by presupposing that each fibre in the auditory nerve has its special function, breaks the most elementary law we know regarding the nature of nerve constitution.

Recent advances in our knowledge of the evolution of the internal ear throw a most definite light on the mechanism of the cochlea and organ of Corti. The ear has been evolved from the balancing apparatus of the primitive labyrinth; the principle which has been adopted by Nature in working out the organ of hearing is merely an extension of the principle used in the primitive labyrinth. In the lowest fishes a closed vesicle on each side of the head, filled with fluid, serves as the central part of the labyrinth; on its floor is a nest or island of hair-cells. On the hairs is balanced an otolith: nerve fibrils commence in or round the hair-cells. So long as a fish swims on an even keel the ciliary semaphoric system is at rest; but if it heels over, ever so slightly, then gravity comes into play; the otolith as it answers to gravity bends the hairlets right or left, as the case may be, and on bending the hairlets sets up certain tensions or changes in the living cells to which they are attached, and these changes are transmitted as signals or impulses along the attached nerves. In this simple semaphoric apparatus there are four elements: (1) the otolith or *titillator*; (2) the hairlet or lever on which the titillator acts; (3) the sense-cell on which the lever acts; (4) the nerve-fibres which are acted upon or stimulated by the sense-cells.

In the sense organs or signal stations of the semicircular canals which have been evolved for the registration of body-movements we find the same four elements. The cupola represents the titillator, but it is no longer acted upon by gravity but by mass movements of fluid set up in the canals during movements of the head. Barány was the first to show that movement of the fluid in one direction gave one set of signals; movement in the reverse direction another and reverse set of signals.

With the evolution of the cochlea and the organ of hearing the same four elements were used. The titillator is the tectorial membrane; the hairlets or levers, the sense-cells and nerves are as before, save that the sense-cells are now set in an elastic scaffolding of fine elastic rods and fibres. But one novel change has been introduced; in the balancing apparatus of the vestibule the sense-cells are fixed; the titillator is movable. In the cochlea Nature has reversed the arrangement and set the sense-cells on a movable membrane—the basilar membrane, which responds to every displacement of fluid set up by waves of sound impinging on the inner ear. On the other hand, the titillator is no longer free but is tethered to the containing wall. Thus in the utricular system the hairlets or levers are worked by gravity; in the canalicular system, mass displacements of fluid set up by movements of the head bend the levers and give rise to signals; in the cochlea the force employed in working the lever system is the minute displacements set up by sound waves, and the levers are bent by the field of hair-cells working against the titillator or tectorial membrane.

The essential modifications, required to make the otic primitive vesicle into an organ of hearing, were a closed vesicle, filled with fluid and everywhere surrounded by bone of a peculiarly dense nature—all except at one area—where a minute window, the fenestra rotunda, was established. This window is essential, for without it there can be no mass displacement of the fluid and no production of nerve-stimuli as sound waves sweep through the bony walls of the vesicle. In the passage leading to the round window is placed the organ of Corti—the apparatus for recording the displacements of fluid set up by the bone-conducted sound waves. To make the ear a more sensitive machine another window has been established in the bony wall of the vesicle—the fenestra ovalis, into which is fixed a movable piston, the stapes. By a bent lever, formed by the ossicles of the ear, this piston is yoked to the membrana tympani, and thus the ear is rendered infinitely more sensitive to sound impulses carried by the air. The area of the drum is fifteen times that of the foot-plate or piston of the stapes. Closure of the fenestra ovalis, by fixation of the stapes, renders the ear more sensitive to bone-conducted waves; closure of the fenestra rotunda produces complete deafness. These facts cannot be explained on the hypothesis put forward by Helmholtz, but find a complete answer in the theory put forward by Sir Thomas Wrightson.

Four phases are to be recognized in the completed movement of the lever or hairlet of a sense-cell. Its upright or vertical position may be regarded as one of rest—its zero position. In the first phase of a complete movement the hairlet bends towards one side—towards the right we shall suppose; in the second it returns to its upright or zero position; in the third it bends towards the left; in the fourth it again returns to its starting or zero point. It is clear that different conditions of tensions and pressures will be set up within the hair-cell in each of these four phases, and each phase we may postulate gives rise to a nerve impulse or signal; the signals set up will vary with the duration and force of each hairlet movement. In each sound wave Sir Thomas Wrightson recognizes four corresponding phases: two of these lie in the part of the wave where the air particles are being condensed—the part in which there is a *plus* pressure; two of them lie in the part where the air particles are being rarefied—where there is a *minus* pressure. In phase I the plus pressure is rising; in phase II the plus pressure is falling; in phase III the minus pressure is increasing; in phase IV the minus pressure is decreasing. Each phase of a sound wave produces a definite action on the hairlets or levers, and thus gives rise to a separate signal or nerve message.

Sir Thomas Wrightson's original discovery, announced in 1876, was the recognition of the fact that, if it could be supposed that each phase of a sound wave did give rise to an effective stimulus in the ear, then the brain was supplied, through the ear, with a sufficiency of data to give a complete analysis of the most complex sound. Helmholtz had supposed that such an analysis could be accomplished only on the principle of resonance; Sir Thomas Wrightson showed that there was an alternative method, one in which the cochlea acts as a whole, weighing and registering the pressures produced by sound waves.

That each phase of a sound wave is effective in producing a distinctive movement of the auditory hairlets was a later discovery, but forms a very essential part of Sir Thomas Wrightson's theory. It was a sequel to a neglected discovery of Sir William Bowman's, made somewhere about the year 1846, that the basilar membrane is composed of two parts—a striate zone and a hyaline zone; the hyaline zone resembles the capsule of the lens in structure and in staining reaction, and must be regarded as elastic in nature. Sir Thomas Wrightson has demonstrated that the displacements which sound waves set up in the fluids within the ear act against the elastic resistance of the basilar membrane, and that thus each of the four phases of a sound wave, which he had originally postulated on theoretical grounds, do thereby become effective in producing a separate and distinctive movement of the hairlets. In Prof. Keith's opinion the various parts of the cochlea, of the organ of Corti and the conformation of the various liquid passages of the ear, which were left unaccounted for on Helmholtz's theory, now receive a satisfactory explanation. He was also convinced that when physiologists, psychologists, and aural surgeons have mastered the details of the new theory they will find themselves provided with clues to phenomena which were formerly inexplicable.

[A. K.]

WEEKLY EVENING MEETING,

Friday, March 28, 1919.

General E. H. Hills, C.M.G. D.Sc. F.R.S.,
Secretary and Vice-President, in the Chair.

The Rt. Hon. Sir John H. A. Macdonald, G.C.B. LL.D. F.R.S. M.I.E.E.

The Air Road.

[Abstract.]

The use of the Air as a Road occupied many minds in past times. Both the ideas of travelling by the aid of light gases, and by the use of force in imitation of birds, were matters of research as far back as the Christian era. Time will not permit that the history of the many devices used be given, but reference may be made to a few. Roger Bacon, in the thirteenth century, wrote of one who adumbrated a balloon of very thin copper, to be filled with—to use his own words—"ethereal air or liquid fire." In the seventeenth century, Francesco Lana proposed to attach four such thin globes of copper to a boat, and these, having been exhausted of air, he thought would ascend. He forgot that as the air had been taken out of the globes they would be "crushed-in" by external atmospheric pressure, and that there would be no buoyancy.

It was left to the Montgolfiers in the eighteenth century to produce the first balloon by using air made light by heating. This was soon followed by the invention of the gas balloon, which for long was the only means of aerial ascent. Its only practical use was for military observations. Such balloons were used at the battles of Maubanji and Fleurus, and by Napoleon in his Egyptian Expedition. These balloons, however, were held to the ground by anchored cables; the defect of the balloon was that it could not be controlled.

There is no time to speak of the many who thus struggled on against learned discouragement. But two sets of investigators call for special notice, as by their indomitable patience and inventive power, they overcame the chief difficulties that strewed the path towards success. They set themselves to attain that which Leonardo da Vinci had enforced long ago, but which had been too much overlooked, perceiving that the first thing essential to safe flight was a knowledge of how the flying-machine could be controlled and its balance maintained, or if lost, recovered. At the risk—a great one I confess—of being considered unpatriotic by giving praise to any person or thing of German origin, I must state the truth, that to two German lads, Otto and Gustav Lilienthal, the honour belongs of having been the first to take this course resolutely. and to study thoroughly balancing and control. So far back as 1885, when only fourteen and twelve years of age, they took aviation up with enthusiasm, and experimented, working in secret to escape the jeers of their schoolmates. With resolute perseverance they made a careful study of the action of birds. Otto put some young storks into an enclosure, and watched them as they endeavoured to fly, observing how they always placed themselves so as to start against the wind, and that when they came down, after blunderingly covering a few yards, they always turned round and walked gravely back, and again faced the wind for another attempt. Instinctively, they sought the pressure of the air to hold up against gravity. In the light of their observations, these lads made many experiments with various models; and ultimately, so that they might study control and balancing, constructed a wing-like frame which would enable a "glide" to be made from a height, the spread-out wings retarding the action of gravity. After using this "glider" from a height, to enable them to set their "glider" to any wind they erected an artificial mound, 50 feet high, sloping in all directions, so that they could always face the wind pressure to aid in gliding for some distance clear of the ground. It will give some idea of the work necessary for making this mound when I say that it contained more than 480,000 cubic feet of soil. When using the "glider," the controller lay on his elbows and breast on a flat surface so that his limbs might hang freely down. "Taking off" from the mound, the wind partially sustained the weight, and he was able to glide free of the ground, and could study control to keep the balance. When any tendency to lose equilibrium by a change of wind was encountered, young Lilienthal restored the balance by moving his body in a counteracting direction. His swinging was the same in effect—only from below instead of from above—as that of the lady performer on a horizontally-stretched wire. In this way, and in spite of wind changes, these lads learned how the downward glide from their mound could be made safely. and gained confidence in the knowledge that if mechanical power could be used to push the "glider" forward it could also be controlled by suitable appliances so that practical flight would be possible. Unfortunately, when at the very threshold of success. their work ended in tragedy. On his last flight Otto neglected to attach the strong spring shock-absorbers, which would mortify the shock of concussion should any accident occur, and on losing his balance, the swing of his limbs being insufficient to restore equilibrium, he fell and was killed. He left behind him a fame that cannot die, and young though he was at the time of his death, he is still spoken of as the greatest pioneer of aviation, and many call him the "Father of the Aeroplane."

There are two other pioneers one is bound to mention. Wilbur and Orville Wright, of America, after studying a book written by Otto Lilienthal, made an aeroplane, driven by one of their own engines, which acted efficiently, and they attained the proud position of being the first to accomplish protracted flight over great distances.

The aeronaut of the day has a duty of gratitude to the Lilienthals and to the Wrights for their brilliant achievements as pioneers of aviation. The first complete knowledge of the essential condition of flying was made by Lilienthal; the first practical flight, in accordance with this condition, by the Wrights. Their work and patience led to the efficiency of to-day.

[In the course of his Discourse the Lecturer referred to the work of Dumont, Sir George Cayley, Henson, Phillips, Wenham, Sir Hiram Maxim, Ader, Professor Langley, Charles Rolls, Bleriot and Hawker.]

The Road of the Air is the medium in which all the work of the inventors has to show its efficiency, for in mechanical flight it is the air which, while it makes flight possible, yet gives rise by its vagaries to the many difficulties to be encountered. Thus the effort to accomplish flight through the air safely has necessarily led to a closer study of the densities, the temperatures, the humidities and the motions of the "invisible road" than had hitherto been made.

There can be no doubt that the Air Road has passed from the region of the imaginative to the region of the practical. Much that was deemed impossible has been accomplished, and there is promise of greater things in the immediate future.

[J. H. A. M.]

WEEKLY EVENING MEETING,

Friday, April 4, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

PROFESSOR FREDERIC HARRISON, D.C.L. D.Litt. LL.D.

History of the City of Constantinople.*

658 B.C.—1919 A.D.

[SUMMARY.]

UNTIL our own age historians ignored, if they did not deny, the services to mankind which the Roman Empire on the Bosphorus secured to the West during the eleven centuries which separate the first Constantine from the last. For 1100 years Constantinople stood a veritable Rock of Ages to preserve the continuity of ancient civilization, the organism of government in peace and war, the tradition of the arts, the literature and science of the world, the power of Eastern Christendom. It drove back Persians, Saracens, Ottomans, and between the attacks of these tremendous enemies incessant onslaughts from Goths, Huns, Avars, Russians, Bulgars, the pirates of the Mediterranean, and swarms of barbarous races.

Finlay, as Lord Morley says, "first unfolded what the Byzantine Empire was." In 1855 Professor Freeman took up the cause. Professor Bury then founded a School of Byzantine Literature. Lord Bryce, Professor Oman, Sir Edwin Pears, Sir Thomas Jackson have illustrated the art and literature of Byzantine ages.

The Eastern Roman Empire lasted in time for 1120 years, and in space once reached from Spain to the Caspian Sea. The Turkish Empire has ruled from Constantinople for 466 years, and once extended from the Upper Danube to the Red Sea and the Persian Gulf. Constantinople indeed has been a historic city for 2577 years, and has been the continuous seat of Empire for 1589 years. During the Middle Ages it was the largest, most civilised, most artistic and lettered city in Europe. As Gibbon says, it was formed by Nature to be the centre and capital of a great power. It was the first great example of "Sea-Power," because it held the straits both north and south which protect the city and its spacious harbours. It has still, by consent of ancients and moderns, the most fascinating site and the most beautiful landscape of any city in the world. Its foundation in 658 by a colony of Greeks led to its connection with most of the great events and most famous men of Greek history. The transfer of the capital of the Roman Empire from the Tiber to the Golden Horn, effected by Constantine the Great in 330 A.D., was one of the master-strokes in the history of civilization.

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* A full Report of the Discourse appears in "The Fortnightly Review" for June, 1919.

The New Rome of Constantine was a splendid city extending three miles inland. Theodosius II. raised a still vaster wall from the Marmora to the Golden Horn four miles from the headland, the circuit we see to-day. Anastasius built a farther wall some fifty miles long from the Marmora to the Black Sea. This tremendous fortification protected Christian and Græco-Roman civilization from the barbarians of the North and the infidels of the East for a thousand years. The walls of Constantinople are the most instructive remains of the military system of the ancient world. It withstood some twenty sieges, and was never pierced until Mahomet II. in 1453 overwhelmed the city by his new cannon. The ruins are still one of the most impressive sights on earth. With the Hippodrome, and its obelisk, its Colossus, and its Delphic column, these are the most venerable relics of antiquity. The glory of Constantinople is the great Church of S. Sophia, built by Justinian in 537 A.D., which with its marbles and mosaics has a radiant majesty entirely its own.

More than twenty ancient churches still remain converted into mosques, as well as the same number of mosques built since the Conquest, some of them almost equal to S. Sophia in size and splendour. Then comes the old Serai, the palaces of the Sultans, rich with endless memorials of glory, art, and crime. The antiquities of Stambul are inexhaustible, and the two modern museums contain treasures, especially in Greek sarcophagi, in bronzes, porcelains, and tapestries that are hardly to be matched in Europe.

The material marvel of Constantinople is its position between Europe and Asia, dominating the whole eastern side of one continent, and also the seas and islands of the Levant. This is as true to-day as in that of Constantine. The moral marvel of the city is the power of revival it has shown since Roman times. What will be its future remains the fateful secret of the League of Nations, to which it presents a problem so complex and so full of great possibilities.

[F. H.]

GENERAL MONTHLY MEETING,

Monday, April 7, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Miss A. Campbell,
C. B. D. Campbell,
Mrs. E. H. Campbell,
J. D. Campbell,
R. G. Campbell,
Mrs. Ernest Davies,
Miss Helen Mary Douglas,
George Keith Buller Elphinstone,
John William Evans, LL.B. D.Sc.
Charles Worthington Hawksley,
Captain Arthur John Hollick,
Rt. Hon. Lord Leigh, M.A.
Miss Irene Henrietta Mond,
Robert Andrew Yule,

were elected Members of the Royal Institution.

The Chairman announced the decease, on April 2, of Sir James Mackenzie Davidson, and the following Resolution passed by the Managers at their Meeting held this day was unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the great loss sustained by the Institution by the death of Sir James Mackenzie Davidson, M.B. C.B.

Sir James Mackenzie Davidson was a Member of the Royal Institution for twenty-two years, and held Office both as Manager and as Visitor, and at the time of his death was a Member of the Managerial Body.

He was the author of many important Papers on Ophthalmology, and devoted much attention to X-Rays and their application to Medical Treatment. He invented the precise method of localizing bullets and other foreign substances in the body by their means, which has proved of such signal service in the present War.

Sir James Mackenzie Davidson gave three Friday Evenings on the following subjects, viz. :—

1. X-Rays and Localization (1902).
2. Vital Effects of Radium and other Rays (1912).
3. Electrical Methods in Surgical Advance (1916).

On behalf of the Members, the Managers desire to express their deep sympathy with Lady Mackenzie Davidson and the family in their bereavement.

The following Lecture Arrangements After Easter were announced :—

PROFESSOR ARTHUR KEITH, M.D. LL.D. F.R.S. F.R.C.S. M.R.I., Fullerian Professor of Physiology, Royal Institution. Four Lectures on BRITISH ETHNOLOGY: THE PEOPLE OF WALES AND IRELAND. On *Tuesdays*, April 29, May 6, 13, 20.

PROFESSOR W. H. BRAGG, C.B.E. D.Sc. F.R.S., Quain Professor of Physics, University of London. Two Lectures on LISTENING UNDER WATER. On *Tuesdays*, May 27, June 3. (The Tyndall Lectures.)

H. S. HELE-SHAW, Esq., LL.D. D.Sc. F.R.S. M.Inst.C.E. M.R.I. Two Lectures on CLUTCHES. On *Thursdays*, May 1, 8.

PROFESSOR FREDERICK KEEBLE, C.B.E. F.R.S. Two Lectures on HORTICULTURE. On *Thursdays*, May 15, 22.

SIR VALENTINE CHIROL. Two Lectures on THE BALKANS. On *Thursdays*, May 29, June 5.

PROFESSOR H. S. FOXWELL, F.B.A., Professor of Political Economy, University of London, Fellow and Director of Economic Studies, St. John's College, Cambridge. Two Lectures on CHAPTERS IN THE PSYCHOLOGY OF INDUSTRY: 1. FOURIER AND OTHER PIONEERS IN THE MOVEMENT FOR THE HUMANIZING OF INDUSTRY; 2. MODERN INDUSTRIAL ORGANIZATION: WHERE IT FAILS TO OBSERVE THE HUMANITIES OF INDUSTRY, AND THE RESULT. On *Saturdays*, May 3, 10.

J. WELLS, Esq., M.A., Warden, Wadham College, Oxford. Two Lectures on: 1. CÆSAR'S PERSONAL CHARACTER AS SEEN IN HIS COMMENTARIES; 2. CÆSAR AS A GENERAL. On *Saturdays*, May 17, 24.

JULIUS M. PRICE, Esq., F.R.G.S., War-Artist-Correspondent of the "Illustrated London News" with the Italian Army. Two Lectures on THE ITALIAN FRONT. On *Saturdays*, May 31, June 7.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Journal of India, Vol. XIV. Part 1. 8vo. 1919.
Report on an Inquiry into the Silk Industry in India. By H. Maxwell Lefroy and E. C. Ansorge. 3 vol. 4to. 1917.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta, Rendiconti: Classe di Scienze Fisiche, Matematiche e Naturali, Vol. XXVIII. 1° Sem. Fasc. 1–2. 8vo. 1919.
American Geographical Society—Geographical Review, Jan.-Feb. 1919. 8vo.
Asiatic Society, Royal—Journal, Jan. 1919. 8vo.
Australia, Commonwealth of, Advisory Council of Science and Industry—Memoirs, No. 1. The Australian Environment. By G. Taylor. 4to. 1918.
Bankers, Institute of—Journal, Vol. XL. Parts 3–4. 8vo. 1919.
Belgium, Royal Academy of Sciences—Rapport sur l'état du Palais des Académies après le départ des Allemands. 8vo. 1919.
Rapport sur la Vie Académique, 1914–18. 8vo. 1919.
Tables Générales des Bulletins, 4e Série, 1899 à 1910; 5e Série, 1911 à 1914. 8vo. 1919.
Table des Notices Biographiques publiées dans l'Annuaire, 1835–1919. 8vo. 1919.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXVI. No. 5. 4to. 1919.
British Astronomical Association—Journal, Vol. XXIX. No. 4. 1919. 8vo.
British Dental Association—Journal, Vol. XL. Nos. 5–6. 8vo. 1919.
Buenos Aires—Bulletin of Statistics, Nov.–Dec. 1918. 4to.
Canada, Department of Mines—Mineral Production, 1918: Preliminary Report. 8vo. 1919.
Carnegie Institute of Washington—Annual Report on Terrestrial Magnetism, 1917. 8vo. 1919.
Chemical Industry, Society of—Journal, March 1919. 8vo.
Chemical Society—Journal and Proceedings, Feb.–March 1919. 8vo.
Chili, Instituto Central Meteorologico—Anuario Meteorologico, Parts 1–2, 1915. 4to. 1916.
Colonial Institute, Royal—United Empire, Vol. X. March 1919. 8vo.
Cornwall Polytechnic Society, Royal—Eighty-Fifth Annual Report, N.S. Vol. II. Part 1. 8vo. 1918.
Editors—Aeronautical Journal, Jan.–Feb. 1919. 8vo.
Athenæum, March 1919. 4to.
Author, March 1919. 8vo.
Chemist and Druggist, March 1919. 8vo.
Church Gazette, March 1919. 8vo.
Dyer and Calico Printer, March 1919. 4to.
Electrical Times, March 1919. 4to.
Engineer, March 1919. fol.
Engineering, March 1919. fol.
General Electrical Review, Feb. 1919. 8vo.
Horological Journal, March 1919. 8vo.
Illuminating Engineer, Nov.–Dec. 1918, Jan.–Feb. 1919. 8vo.
Journal of Physical Chemistry, Jan. 1919. 8vo.
Junior Mechanics, March 1919. 8vo.
Law Journal, March 1919. 8vo.
London University Gazette, March 1919. 4to.
Model Engineer, March 1919. 8vo.
Musical Times, March 1919. 8vo.
Nature, March 1919. 4to.
New Church Magazine, March–April 1919. 8vo.
Nuovo Cimento, July–Aug. 1918. 8vo.
Physical Review, Feb. 1919. 8vo.
Science Abstracts, Feb. 1919. 8vo.
Terrestrial Magnetism, Dec. 1918, March 1919. 8vo.
Wireless World, March–April 1919. 8vo.
Zoophilist, March–April 1919. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LVII. Nos. 279–280. 1919. 8vo.
Faraday Society—Transactions, Vol. XIV. Parts 1–2. 8vo. 1919.
Firenze, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani, Jan. 1919. 8vo.
Franklin Institute—Journal, March 1919. 8vo.
Geneva, Société de Physique—Mémoires, Vol. XXXIX. Fasc. 2. 4to. 1919.
Geographical Society, Royal—Journal, March–April 1919. 8vo.
Geological Society of London—Abstracts of Proceedings, Nos. 1034–1037. 8vo. 1919.
Horticultural Society, Royal—Journal, Vol. XLIII. Parts 2–3. 8vo. 1919.
Iron and Steel Institute—Journal, Vol. XCVIII. 8vo. 1918.
London County Council—Gazette, March 1919. 4to.
Meteorological Office—Daily Readings, Jan. 1919. 4to.
Circular No. 34, April 1919. 8vo.

Microscopical Society, Royal—Journal, 1918, Part 4. 8vo.
Newfoundland High Commission—Newfoundland Guide Book, 1911. 8vo.
Newfoundland Year Book, 1915. 8vo.
Views of Newfoundland. 8vo.
Newfoundland Geological Survey: Various Reports, 1881–1903. 1917. 8vo.
Report of Agricultural Board, 1917. 8vo. 1918.
Crown Lands Acts, 1903–1911. 8vo. 1912.
New York, Society for Experimental Biology—Proceedings, Vol. XVI. No. 3. 8vo. 1919.
Numismatic Society, Royal—Numismatic Chronicle, 1918, Part 2. 8vo.
Peru, Cuerpo de Ingenieros de Minas—Boletin, Nos. 93–94. 8vo. 1918.
Pharmaceutical Society of Great Britain—Journal, March 1919. 8vo.
Photographic Society, Royal—Journal, March 1919. 8vo.
Physicians, Royal College of—List of Fellows, etc., 1919. 8vo.
Radcliffe Library, Oxford—Catalogue of Books, 1918. 8vo. 1919.
Rome, Minister of Public Works—Giornale del Genio Civile, Dec. 1918–Jan. 1919. 8vo.
Röntgen Society—Journal, Vol. XIV. Nos. 55–58. 8vo. 1918–19.
Royal Engineers' Institute—Journal, Vol. XXIX. Nos. 3–4. 8vo. 1919.
Royal Society of Arts—Journal, March 1919. 8vo.
Royal Society of London—Philosophical Transactions, A, Vol. CCXVIII. No. 563. 4to. 1919.
Proceedings, A, Vol. XCV. No. 670. 8vo. 1919.
Year Book, 1919. 8vo.
Salford, County Borough of—Seventieth Report of Museums and Libraries Committee, 1917–18. 8vo. 1919.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXV. Nos. 3–4. 8vo. 1919.
Società degli Spettroscopisti Italiani—Memorie, Serie 2, Vol. VII. Disp. 10–12. 4to. 1918.
Societatis Liturgicæ Sancti Willibrordi—Acta, Anno 1918. 8vo. 1919.
South African Association for the Advancement of Science—Report of 15th Annual Meeting, 1917. 8vo. 1918.
Statistical Society, Royal—Journal, Vol. LXXXII. Part 1. 8vo. 1919.
Swiss Chemical Society—Helvetica Chimica Acta, Vol. II. Fasc. 2. 8vo. 1919.
Tôhoku Imperial University, Sendai, Japan—Science Reports, 1st Series, Vol. VII. No. 3. 4to. 1918.
United States Department of Agriculture—Journal of Agricultural Research, Vol. XV. No. 12; Vol. XVI. Nos. 2–4. 8vo. 1918–19.
United States Patent Office—Official Gazette, Vol. CCLVIII. Nos. 1–5. 8vo. 1919.
Washington, National Academy of Sciences—Proceedings, Vol. V. No. 2. 8vo. 1919.
Zurich Naturforschenden Gesellschaft—Vierteljahrsschrift, Band 61, Heft 3–4; Band 62; Band 63, Heft 1–2. 8vo. 1916–18.

ANNUAL MEETING,

Thursday, May 1, 1919.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1918, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Twenty-four new Members were elected in 1918.

Sixty-three Lectures and Nineteen Friday Discourses were delivered in 1918.

The Books and Pamphlets presented in 1918 amounted to 171 volumes, making, with 404 volumes (including Periodicals bound) purchased by the Managers, a total of 575 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :

President—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.
Treasurer—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.
Secretary—Colonel E. H. Hills, C.M.G. R.E. D.Sc. F.R.S.

Managers.

H. E. Armstrong, LL.D. F.R.S.
Sir J. Phipson Beale, Bart., K.C. M.P.
Horace T. Brown, LL.D. F.R.S.
J. H. Balfour Browne, K.C. LL.D.
J. A. Fleming, D.Sc. F.R.S.
J. Dundas Grant, M.D. F.R.C.S.
Colonel J. Gretton, M.P.
The Rt. Hon. Viscount Hambledon.
Dr. Donald W. C. Hood, C.V.O. M.D.
H. R. Kempe, M.Inst.C.E.
The Hon. Sir Charles Parsons, K.C.B. F.R.S.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D.
J. Emerson Reynolds, M.D. F.R.S.
James Swinburne, F.R.S.
Sir Thomas Wrightson, Bart., J.P.

Visitors.

Sir W. H. Bennett, K.C.V.O.
W. R. Bousfield, K.C. F.R.S.
John G. Bristow, M.A.
Frank Clowes, D.Sc., F.C.S.
Edward Dent, M.A.
Sir James J. Dobbie, LL.D. F.R.S.
J. Hunter Gray, K.C. B.Sc.
W. A. T. Hallowes, M.A.
F. K. McClean, F.R.A.S.
W. R. Malcolm, M.A.
T. C. Owen.
Richard Pearce, F.G.S.
Sir Edward E. Pearson, J.P.
Hugh Munro Ross, B.A.
Joseph Shaw, K.C.

WEEKLY EVENING MEETING,

Friday, May 2, 1919.

General E. H. Hills, C.M.G. D.Sc. F.R.S.,
Secretary and Vice-President, in the Chair.

Professor John W. Nicholson, M.A. D.Sc. F.R.S.

Energy Distribution in Spectra.

Our knowledge of the structure of very fine spectrum lines is now on a secure and nearly comprehensive basis, from the point of view both of theory and of experiment, and it is very remarkable that so little is known of the distribution of energy in these lines, either in individual lines, or as between the different lines contained in the spectrum of any atom. The analysis of the spectra of atoms, theoretically and in the laboratory, is now recognised as the most critical test to which any theory of atomic structure can be subjected, and we have recently had theories of the atom entirely based upon the wave-lengths of the radiations which they emit. A question of equal importance, to which I now wish to direct your attention, is the relative amounts of energy which the atom throws out in the different wave-lengths, for this is well known to vary in some cases very greatly, according to the circumstances under which the atom is excited. I shall describe, in the first instance, a method designed by Dr. Merton for investigating the distribution of energy among spectrum lines, or in the breadth of an individual line, with great accuracy. It is possible by this means to obtain the long-desired object of an absolute scale of spectral intensity, independent of all the ordinary difficulties determined by such matters as the unequal behaviour of the photographic plate for light in different regions of the spectrum. Dr. Merton and I have been working together on this subject for the past three years, and I shall conclude the present discourse with an account of some of the more interesting results which have been reached, after an explanation of the method.

The intensities of spectrum lines have usually been recorded on an arbitrary scale, ranging between 10 and zero, the numbers assigned being at the discretion of the observer, and varying so greatly among different observers as frequently to be of little value for exact knowledge. They depend also very much on the nature of the observation, whether visual or photographic, and in the latter case, on the region of the spectrum to which the line belongs. The sensitivity of a photographic plate varies with the wave-length of the light in a curious manner, and apparently an irregular one not following any simple law. The sensitivity of the eye is also different for different colours.

When the line is outside the visible spectrum, in the infra-red or dark heat region, measurements of intensity can be made with some accuracy by a thermopile or a bolometer. But they are needed more urgently in the visible region at present, not only for the information they will afford regarding the nature of the atom, but also for application to other problems. The subject is very important, for instance, in the interpretation of celestial spectra, and more particularly those spectra of great complexity and variability which are associated with the birth of new stars, from which most of our knowledge regarding such stars must be constructed.

Previous knowledge of changes in spectral intensity under varying conditions was of necessity limited to the great changes. Those changes which are of especial value in connections such as I have mentioned are liable to be of a less conspicuous type, not readily capable of detection by the ordinary photographic or the visual method, and if detected, not capable of accurate measurement.

In the visual region of the spectrum, observations with the bolometer are not satisfactory. The source of light must be very intense in order to produce large deflections in the galvanometer, and only the brightest lines could be dealt with even in this way. Only one line in the spectrum can be experimented upon at a time, and the source of light cannot be maintained constant over a protracted period. The method is in fact quite unsuitable, and the spectrophotometer has been tried instead, but no great accuracy is possible, and its use is confined to a very narrow region of wave-length. Moreover, the variability of the source of light is again present.

In adopting any photographic method for quantitative work, we must remember that not only does the sensitivity of the plate vary with the wave-length, but also that there is no very definite relation between the density of a photographic image and either the intensity of the light or the time of exposure. If we halve the former and double the latter, we do not get the same density of the image, but another which depends on the particular plate used. The grain of a plate also scatters light, and the critical size of the image thus depends on the exposure and the intensity of the light. We were early compelled to conclude that accurate measurements of intensity by a photographic method involve the necessity of an equal exposure on the same plate for all the sources of light to be compared, and the method to be described satisfies this necessity.

The spectrograph for producing and photographing the lines of a spectrum is set up in the usual way, which requires no description. A wedge of neutral-tinted glass, cemented to another of clear glass so as to form a plane-parallel plate, is mounted in front of the slit. The image of the slit formed by light of any wave-length is thus attenuated towards the part of the slit opposite the thick end of the wedge, where the absorption of light is greatest, and the image ceases to be strong enough to affect the plate beyond a certain specific height which depends on the original intensity, in the beam from the source, of this particular wave-length.

The photograph thus consists, not of the usual spectrum with all lines or slit-images of the same length, but of a spectrum in which all the lines are cut down to specific heights depending on the original intensities, and thus it gives a simultaneous record of all the intensities in the spectrum at any one instant. All spectrum lines have a breadth, due to Doppler effect of the atomic motions in the kinetic theory, and to other agencies. The shape of one of the truncated lines depends on the original law of intensity across the line. This shape may be a wedge, or may be bounded by a more or less rounded curve, from whose nature, if the boundary can be defined sharply, we can deduce mathematically the law of intensity across the original line. Sharp changes of intensity, such as occur when the line has several close components overlapping one another, are detected as peaks or kinks in this bounding curve. The original photograph can be enlarged with considerable magnifying power, and if the bounding curve on this enlargement is sharply defined, we can obtain its mathematical shape very accurately, and deduce an estimate of the intensity in any part of the line with a great degree of precision. We have been able to show, for instance, that in most of our experiments, such accuracy as one part in 100 has been reached, and it could be increased readily if desired by the use of greater magnification of the original photograph.

The determination of the exact boundary of a patch of dark on a white ground is a matter in which "personal equation" is important. We overcame this difficulty by enlarging positives, prepared from the negatives, on to bromide paper through a ruled "process" screen. The resulting photograph consists in this way af an assemblage of very minute dots, fading away towards the boundary into invisibility. It is a simple matter to prick out the last dots visible all round the contour, and in this way personal equation can apparently be entirely eliminated. We adopted usually about 100 dots to the inch on the final photograph. If comparisons of different lines with the another are required, only the central heights of the figures are necessary, and the topmost dot can be seen at once.

The first application of the method was to the intensity distribution in the lines of the hydrogen spectrum when a condensed discharge was passed through the exciting tube. It was known that with a condensed discharge the lines always appeared much broader, and we concluded that the best method of obtaining information as to the source of the effect was to examine the intensity distribution across the lines. Some remarkable contours were obtained, showing at once a clear distinction between this source of broadening and that associated with the Doppler effect. The contours associated with the latter should be thin symmetrical parabolas. Those we found were wedge-shaped, with definite kinks indicating the introduction of new component lines when the condenser was put into the circuit. The wedge-shape indicates that the law of decrease of intensity from the centre of a component is expotential, and not the law of error as in the Doppler effect. By measuring the distances between the kinks, and knowing the magnification and a previous calibration of the wedge, all necessary quantitative data of the spectrum line can be calculated. It was possible to show, in particular, that the separations in wave-length of the components of H_a were those found by Stark when new components were called into being by the existence of an enormous external potential gradient. As we had suspected, the origin of this exceptional broadening under the condensed discharge is the "electric Zeeman effect," the origin of the large electric field on any atom being the close proximity of other charged atoms. We thus have a new means of studying the electrical resolution of spectrum lines, more convenient in many ways than the older methods, and capable of much greater generality and accuracy. A large number of observations of the same phenomenon were made also on the spectrum lines of helium and lithium, and the correspondence with the Stark effect always held good.

The examination of an individual line has also been applied in the case of an "ordinary" discharge, and given the first direct proof of the probability distribution of velocities in the radiating atoms of a gas. This distribution has been taken as a basis by Lord Rayleigh in the elaboration of a precise method of determining the mass of a radiating atom from the breadths of the spectrum lines, a method applied by Buisson and Fabry with great success, when the breadth is measured by methods of interference of light. Our experiments have defined very closely the circumstances in which this method is practicable, and shown that it fails altogether if condensed discharges are employed. In the ordinary uncondensed discharge under low pressure, however, our contours are very accurately parabolic, which fact can be shown to imply a very rigorous probability law of velocities in the atoms, and no other important source of broadening of lines in these circumstances.

The only other application to an individual line which I shall mention concerns the nature of the Balmer series of hydrogen, long believed to be a Diffuse Series, with each line consisting of two close components, hardly separable or not separable at all, with the same interval in frequency between them for every line. We have shown that it is in fact a Principal Series, with the separations decreasing in a calculable way required by theory, conforming also the separation in H_a given by Buisson and Fabry. The method was to use the neutral wedge in combination with another apparatus of extreme resolving power—in this case a Lummer-Gehrcke plate. We can in this way obtain contours for a pair of close components which cannot be detected visually as a pair, and the actual interval can be deduced by a series of measurements of the joint contour, consisting of two overlapping parabolas. We calculate the position of the vertex of the inner one, and thence the separation, which can, in H_β, be determined within about 0·001 of an Ångström unit. The actual separation in this line is as small as 0·030 Å., and the present method could measure accurately a much smaller separation.

We pass now from the phenomena of structure and intensity of a single line to those involving a comparison of different lines. Here the behaviour of the plate for different wave-lengths must be dealt with. But it so happens that every plate can be calibrated by throwing on to it not only the whole spectrum under examination, but the radiation—a continuous spectrum—from the positive crater of the carbon arc. The energy distribution in this case is known from Wien's law when the temperature of the arc is known, as it is very closely. On the slide you will notice the curious contour bounding this spectrum, largely due to vagaries in the sensitivity of the plate. Above it is the helium spectrum on the same plate. To obtain an absolute scale of intensities down the helium spectrum, independent of all sources of error due to apparatus, we only need to compare the heights of the lines with the corresponding heights directly below them in the carbon-arc spectrum. It is in fact logarithms of intensity which the heights represent, and the differences of height represent powers of a definite *factor* entering into the intensities, so that the photographs give no visual impression of the enormous differences of intensity which occur. For example, the line of wave-length λ 3888, a Principal line in the helium spectrum, appears quite short on the photograph, but is actually the most intense in the spectrum. Absorption in the apparatus is strong in this region. One of our conclusions, in fact, is that Principal Series deserve their name even in elements which appeared hitherto to be exceptions, in that they do contain, for the visible region, a preponderant part of the radiated energy.

It is not necessary to use the carbon arc in every subsequent experiment. We can by its means calibrate the helium spectrum under conditions easily reproduced, and subsequently take this as our standard, especially when the work projected is the variation of the helium spectrum under changing conditions of excitation. Some of the remaining slides indicate the unexpected character of some of these variations It would not be possible in this Discourse to give anything like a complete account of the phenomena of this class already investigated, and I shall therefore confine myself to some of those which are most striking. In the first place we may notice the spectrum of a mixture of hydrogen and helium or neon. The fundamental phenomenon which this method has detected is what we have called "transfer of energy along a series." For instance, in the Balmer series of hydrogen, produced from pure hydrogen under "ordinary" conditions, there is a perfectly definite intensity relation among the lines H_α, H_β, and so on; but in the presence of helium this is disturbed, and the ratios H_β / H_α, H_γ / H_β, H_δ / H_γ, . . . are notably increased. In other words, more energy tends to be emitted in the form of the more refrangible rays, at the expense of the less refrangible. It is interesting to speculate as to how far this process can be carried, for its logical outcome is a radiation from hydrogen concentrated at λ 3646, the limit of the Balmer series. We have not in fact examined the matter from this point of view.

Neon produces the same effect on the hydrogen spectrum, the first recorded evidence being an experiment of Liveing and Dewar, who found in 1900 that it was possible to observe more of the violet members of the Balmer series when neon was present. We have made quantitative measurements of the effect in various cases, and in one experiment, for instance, the neon was found to make H_β $\frac{6}{5}$ as strong, H_γ $\frac{9}{5}$ as strong, and H_δ $\frac{11}{5}$ as strong, in relation to H_α. But I shall not enter into further numerical detail. A particularly interesting fact is that the effect of a small trace of an impurity is often diametrically opposite to that of a large quantity, and causes the transfer of energy to take place in the opposite direction along the spectrum. There are evidently two quite different mechanisms of interaction possible between the atoms of the two gases—a problem I commend to the chemist for investigation.

But it is not necessary to mix a gas with another gas in order to produce the energy transfer. It can be achieved otherwise, as some further slides I have here will suffice to show. We have made many measurements of intensity, more especially in the spectrum of pure helium, of the lines from a pure gas as dependent on the part of the tube they arise from, and on the conditions of excitation. We shall only consider one or two of the more interesting results which arise from a comparison of three spectra of helium: (1) the "ordinary" spectrum, or the spectrum given by the capillary of a vacuum tube containing helium at about a millimetre pressure, excited by the discharge from an induction coil without capacity or spark gap; (2) the bulb spectrum, obtained by putting a small condenser and a very small spark gap in parallel in the circuit; and (3) the capillary spectrum, with a spark gap and a strong condensed discharge. In both (2) and (3), the transfer of energy to the more refrangible members of a series takes place very strongly. In the Diffuse Series, the transferred energy goes in (3) mainly towards increased breadth of the line, but in (2) mainly towards enhanced central intensity—two very different effects. The Sharp and Principal Series show the same transfer quite definitely, though on a smaller scale, and the effect in these series is more closely confined to enhancement of the central intensity.

The most striking enhancements produced by the condensed discharge in helium occur with the lines λ 4472 and λ 4388, which are precisely the helium lines apt to be found abnormally strong in the spectra of some of the planetary nebulae. Some other experiments we have made, on the spectrum of helium at very low pressure, indicate that these lines, together with the line λ 5015 more frequently quoted, are the strongly enhanced lines also in these circumstances. If the two sets of circumstances occur together, λ 5015 is not especially strong, but the others are enhanced for both reasons. We have in fact been able to demonstrate that the peculiar "nebular" spectrum of helium could be produced in the laboratory by a combination of the condensed discharge with an extremely low pressure.

I shall not discuss the spectra of gases as dependent, in their intensity relations, on pressure. The time required would be prohibitive, and my object is to indicate the range of work now open to precise investigation, rather than to give any complete account of the phenomena which the method has yet indicated or elucidated. One remark must, however, be made in connection with high-pressure spectra. We investigated the intensity distribution in a helium tube at the extraordinary pressure of 42 mm. Except for the trace of hydrogen which came out of the electrodes during the discharge the helium was pure. Yet the hydrogen spectrum was nevertheless predominant on the plate, and fourteen members of the Balmer series, instead of the usual seven or eight, could be seen visually as very sharp lines. This phenomenon incidentally cannot be reconciled with the current quantum theory of the hydrogen spectrum—perhaps not an unexpected fact to those conversant with the hydrogen spectrum. No spectroscopist can in fact accept a theory which can give no hint of the origin of the so-called "secondary spectrum" of hydrogen, known to arise mainly from the atom, and, in the laboratory at least, the most important and extensive part of the spectrum. The elucidation of this spectrum is in many ways the most fundamental problem of physics, and far more fundamental than the Balmer series problem. No atomic theory as yet has begun to interpret any of this spectrum, and several have given a theoretical account of the Balmer series.

Many of the new problems of interest, which the possession of an accurate method of intensity determination in spectra enables us to attack, are mainly of astrophysical importance. There may be variations of intensity in the Fraunhofer lines accompanying other more readily perceived solar phenomena, for example, but of more urgent importance is the need for a series of photographic registers of the intensity across the whole spectrum of a new star at different stages of its history. It has not often been possible even to determine the actual number of component radiations, in an apparent broad band with a structure, emitted from such a star, at least with any real certainty. A method which automatically sifts out such bands and gives peaks on a photograph at all the maxima of intensity in the band may well be expected to contribute greatly to the elucidation of the phenomena taking place, which must in any case be totally different from anything known by our terrestrial experience.

The only other class of phenomenon depending for its elucidation on precise measures of intensity in spectrum lines, to which I shall refer with further illustrative slides, is the variation which takes place in the spectrum from a helium tube as we recede from the cathode. The slides serve to show the considerable differences which take place in the distribution of the various series, which are all emitted most strongly at unequal distances from the cathode. One very extraordinary result, indicated clearly on the last slide, is the fact that there exists a narrow region of the tube in which the characteristic spark line λ4686 is emitted simultaneously with the helium band spectrum, a circumstance which necessitates some readjustment of preconceived ideas.

[J. W. N.]

GENERAL MONTHLY MEETING,

Monday, May 5, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

His Royal Highness The Prince of Wales, K.G.

was elected an Honorary Member of the Royal Institution.

The Chairman announced that His Royal Highness The Prince of Wales had graciously consented to become Vice-Patron of the Royal Institution.

The Chairman announced that His Grace The President had nominated the following gentlemen as Vice-Presidents for the ensuing year:—

Henry Edward Armstrong, LL.D. F.R.S. F.C.S.
Sir William Phipson Beale, Bart., K.C. M.P. F.C.S.
John Hutton Balfour Browne, K.C. D.L. J.P. LL.D.
Donald William Charles Hood, C.V.O. M.D.
The Hon. Sir Charles Parsons, K.C.B. J.P. M.A. Sc.D. LL.D. F.R.S.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S. (Treasurer).
Brigadier-General Edmond H. Hills, C.M.G. R.E. D.Sc. F.R.S. (Secretary).

Colonel G. Duff Baker, R.A.
Mrs. Vera Bryce,
Ernest Charles Scott Dickson, B.A. Ph.D.
Miss Marion Newton Drew,
Captain Alfred Charles Glyn Egerton,
Lady Glenconner,
Richard Owen Jones, B.Sc.
Kenneth Sutherland Murray,
John Robert Pretyman Newman, M.P.
Mrs. Pretyman Newman,
John Joseph Stewart, M.A. B.Sc.
Mrs. Street,
Mrs. Evelina Whittard,

were elected Members.

The Chairman announced the decease, on April 5th, of Sir William Crookes, and on April 29th, of Sir Frank Crisp, Bart., and the following Resolutions, passed by the Managers at their Meeting held this day, were unanimously adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their deep sense of the great loss sustained by the Institution and the Scientific World by the decease of Sir William Crookes, Member of the Order of Merit, Doctor of Science, Past President of the Royal Society of London, Corresponding Member of the Academy of Sciences, Paris; Foreign Member of the Reale Accademia dei Lincei, Rome; the Royal Swedish Academy of Sciences, and many other Foreign Academies.

He was awarded the Royal, Davy and Copley Medals of the Royal Society; and in 1880 received a special Gold Medal and Prize of 3000 francs from the French Academy of Sciences for his work on "Radiant Matter and Molecular Physics." His original investigations in various departments of Science were fruitful in the discovery of the element Thallium, the invention of the Radiometer, and the isolation of Rare Earths.

The discovery of radioactivity in 1896 led Sir William Crookes to devote much attention to the study of this branch of Science, and he invented the Spinthariscope, an instrument for rendering visible the new rays produced by the spontaneous decomposition of Radium, known as the Alpha, Beta and Gamma Rays.

In his Presidential Address before the British Association in 1886, he drew attention to the vital importance of the artificial fixation of atmospheric nitrogen in connection with the production of wheat for world consumption, and he subsequently published a book entitled "The Wheat Problem," the third edition of which appeared in 1917.

He carried out exhaustive researches on the composition of glass, and was successful in the preparation of a glass opaque to the ultra-violet rays and cutting off heat radiation, for the use of workers exposed to furnace glare.

Sir William Crookes was a Member of the Royal Institution for forty years, and gave ungrudging service successively as Manager, Secretary and Vice-President. In all these offices his love and respect for the Royal Institution were fully expressed by the great work he did on its behalf. He delivered four Friday Discourses on: 1. "The Mechanical Action of Light" (1876); 2. "Molecular Physics in High Vacua" (1879); 3. "The Genesis of the Elements" (1887); 4. "Diamonds" (1897).

The Managers desire to express, on behalf of the Members of the Royal Institution, their sincere sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution through the death of Sir Frank Crisp, Bart., LL.B. B.A. J.P. F.L.S.

Sir Frank Crisp was a member of a distinguished firm of Solicitors, and a leading authority on legal questions connected with industrial matters. He was for many years Honorary Secretary of the Royal Microscopical Society, and Treasurer of the Linnean Society of London. He rendered valuable service to Horticultural Science, and was noted for his world-wide famous gardens.

Sir Frank Crisp was a Member of the Royal Institution for forty-one years, and during that period served in the offices of both Manager and Visitor, and showed his interest in the Institution by the delivery of a Friday Discourse in 1888 on "Ancient Microscopes." He enriched the Library of the Royal Institution by the presentation of Miss Willmott's elaborate work on "The Genus Rosa," in 1914.

On behalf of the Members, the Managers desire to express their deep sympathy with Lady Crisp and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Bulletin of Agricultural Research Institute, Pusa, No. 85. 8vo. 1919.
Trigonometrical Survey: Professional Papers, Nos. 16–17. 4to. 1918.
American Geographical Society—Geographical Review, March 1919. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXIX. No. 4. 8vo. 1919.
Australia, Commonwealth of, Advisory Council of Science and Industry—Bulletin Nos. 9–11. 8vo. 1918–19.
Batavia, Royal Magnetical and Meteorological Observatory—Observations, Secondary Stations, Vol. VI. 1916. 4to. 1918.
Boston Public Library—Bulletin, 4th Series, Vol. I. No. 1. 8vo. 1919.
British Architects, Royal Institute of—Journal, Vol. XXVI. No. 6. 8vo. 1919.
British Astronomical Association—Journal, Vol. XXIX. No. 5. 8vo. 1919.
Memoirs, Vol. XXI. Part 3. 8vo. 1919.
British Dental Association—Journal, Vol. XL. Nos. 7–8. 8vo. 1919.
British Red Cross Society—Fourth Annual Report of the Finance Committee. 8vo. 1919.
Chemical Industry, Society of—Journal, April 1919. 8vo.
Chemical Society—Journal and Proceedings, April 1919 8vo.
Colonial Institute, Royal—United Empire, April 1919. 8vo.
Conjoint Board of Scientific Societies—Second Report of Water Power Committee. 8vo. 1919.
Cornwall Polytechnic Society, Royal—Reports, Nos. 79, 81, 82, 83. 8vo. 1912-16.
Decimal Association—Decimal Coinage. 8vo. 1919.
Industrial Reconstruction and the Metric System. 8vo. 1919.
East India Association—Journal, Vol. X. No. 2. 8vo. 1919.
Editors—Athenæum, April 1919. 4to.
Author, April 1919. 8vo.
Chemist and Druggist, April 1919. 8vo.
Church Gazette, April 1919. 8vo.
Dyer and Calico Printer, April 1919. 4to.
Electrical Times, April 1919. 4to.
Engineer, April 1919. fol.
Engineering, April 1919. fol.
General Electric Review, March 1919. 8vo.
Horological Journal, April 1919. 8vo.
Il Nuovo Cimento, Sept.-Oct. 1918. 8vo.
Journal of Physical Chemistry, Jan. 1919. 8vo.
Junior Mechanics, April 1919. 8vo.
Law Journal, April 1919. 8vo.
London University Gazette, April 1919. 4to.
Model Engineer, April 1919. 8vo.
Musical Times, April 1919. 8vo.
Nature, April 1919. 4to.
Physical Review, March 1919. 8vo.
Science Abstracts, March 1919. 8vo.
Wireless World, May 1919. 8vo.
Zoophilist, May 1919. 8vo.
Firenze, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani, Feb. 1919. 8vo.
Franklin Institute—Journal, April 1919. 8vo.
Geneva, Société de Physique—Comptes Rendus, Vol. XXXVI. No. 1. 8vo. 1919.
Geological Society of London—Abstracts of Proceedings, No. 1038. 8vo. 1919.

Imperial Institute—Bulletin, Vol. XVI. No. 4. 8vo. 1918.

Jordan, W. Leighton, Esq., M.R.I. (*the Author*)—On Payment of the National Debt. 8vo. 1919.

London County Council—Gazette, April 1919. 4to.

Madrid, Real Academia de Ciencias—Revista, Tomo XV. Nos. 10–12; Tomo XVI.; Tomo XVII. Nos. 1–3. 8vo. 1917–18.

Anuario, 1919. 12mo.

Meteorological Office—Daily Readings, Feb. 1919. 4to.

Monaco, Institut Océanographique—Bulletin, Nos. 350–351. 8vo. 1919.

New York, Society for Experimental Biology—Proceedings, Vol. XVI. No. 4. 8vo. 1919.

New Zealand—Census, 1916, Part 10. 4to. 1918.

Peru, Cuerpo de Ingenieros de Minas—Boletin, No. 95. 8vo. 1919.

Pharmaceutical Society of Great Britain—Journal, April 1919. 8vo.

Photographic Society, Royal—Journal, Vol. LIX. No. 4. 8vo. 1919.

Post Office Electrical Engineers, Institution of—Journal, Vol. XII. Part 1. 8vo. 1919.

Provision of a Common Battery Telephone Exchange. By G. F. Greenham.

Royal Engineers' Institute—Journal, Vol. XXIX. No. 5. 8vo. 1919.

Royal Irish Academy—Proceedings, Vol. XXXIV. A, Nos. 5–6; B, Nos. 7–13; C, Nos. 10–11. 8vo. 1918–19.

Royal Society of Arts—Journal, April 1919. 8vo.

Royal Society of London—Proceedings, A, Vol. XCV. No. 671; B, Vol. XC. No. 632. 8vo. 1919.

Philosophical Transactions: A, Vol. CCXVIII. No. 564. 4to. 1919.

Smithsonian Institution—Miscellaneous Collections, Vol. LXIX. No. 10. 8vo. 1919.

Report on the Astrophysical Observatory, 1918. 8vo. 1919.

South Africa, Union of—Department of Agriculture, Bulletin, Nos. 3 & 12. 8vo. 1918.

United States, Department of Agriculture—Journal of Agricultural Research, Vol. XVI. Nos. 5–10. 8vo. 1919.

Experiment Station Record, Vol. XXXIX. Nos. 8–9. 8vo. 1919.

United States Patent Office—Official Gazette, Vol. CCLIX. Nos. 1–4. 8vo. 1919.

Washington, National Academy of Sciences—Proceedings, Vol. V. No. 3. 1919. 8vo.

Western Australia—Quarterly Statistical Abstract, No. 212. 1918. 8vo.

Geological Survey Bulletins, Nos. 67–69, 71–76, with Maps and Sections. 8vo and 4to. 1916–17.

Zoological Society—Annual Report, 1918. 8vo. 1919.

WEEKLY EVENING MEETING,

Friday, May 9, 1919.

SIR JAMES REID, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

SIR GEORGE MACARTNEY, K.C.I.E.

Chinese Turkistan: Past and Present.

CENTRAL ASIA extends roughly from the Caspian Sea on the West to the borders of Manchuria on the East. The whole of this extensive area has many physiographic and climatic conditions in common, whether the particular portion be Russian Turkistan, or Chinese Turkistan, or the north-west corner of Inner China. There is nothing surprising in this when we consider that the area in question is in the temperate zone, hemmed in by lofty ranges of mountains, and is, moreover, at the very heart of the largest of continents. These factors combine to induce great extremes of temperature, a minimized rain-fall and a consequent aridity of soil.

It would be futile to attempt the description of a stretch of country as large as that which lies between the Caspian and Manchuria; but I shall take its middle section, the Lob Basin, as Chinese Turkistan is sometimes called, because those of its rivers that do not lose their way in the sands drain into Lake Lob. Turkistan has somewhat the shape of a wedge, inserted between the Tienshan range and the Kurugh Tagh on the North, and the Kuen-lun and the Karakorum ranges on the South, the point of the wedge resting on the Pamir Mountains just where the "Three Empires meet." The basin itself is an inclined plane, with an elevation on the Pamir side of some 4500 ft. sloping eastwards towards Lob Nor, where the height is no more than 2000 ft. The towns themselves, approximately some twenty-five in number, supporting a population close upon a million, cling close to the skirt of the mountains, dotted on the banks of the rivers as they emerge from the ring of uplands. The centre of the basin is an absolute desert, a wilderness of sand-dunes. What rather strikes the imagination in Turkistan is the close proximity of some of the highest and the lowest points in the world.

The Turkistanis are generally supposed to be an Indo-European race, connected with the stock from which sprang the races of Western Asia and Europe, and though they have from time to time intermarried with the tribes of Huns, Chinese, Tibetans and Kalmaks who have invaded Turkistan, their features, though large and coarse, come closer to those of the European than to those of the Chinese or the Mongol. Their complexion is fair; they are simple and gentle in character, and by no means fanatical, and shut up as they are by their mountain barriers they lead an easy and unenterprising life. In the 13th Century the Turkistanis were Buddhists, but after that time they embraced Islamism, and now they are Mahommedans of the Sunni Sect.

The shrines of the country are the tombs of saints supposed, by the present-day natives, to have been Mahommedans; but not a few of these shrines are in fact older than Islamism, and had been equally sacred to Buddhists. It is a fact worthy of observation that when a people change their religion they do not necessarily change their feelings of veneration for places that have derived their sanctity from the religion cast aside. Much has been talked of lately about the Mosque of St. Sophia at Constantinople. Perhaps in that connection the lesson of the modest shrines of Turkistan should not be overlooked, as it shows that in the history of Asiatic religion the continuity of local worship has often been a significant feature.

In close proximity to the graveyards lie the fields of Turkistan. They are not on flat areas of ground, as in this country, but are cut up into small pieces, each being slightly raised one above the other in the manner of terraces. This is necessary to ensure the ground being properly watered, as the rainfall on the plains is barely two inches in the year—a quantity without any practical use; but the water supply from irrigation canals is abundant, so abundant that in modern times the country scarcely ever suffers from drought. The water supply is drawn from the immense reservoirs of the ice-fields on the summits of the peaks above Kashgar. It is somewhat paradoxical that when the sky is overcast and rain threatens, the fields get no rain, but have to wait for the sun to operate on the ice-fields above.

The export trade of Turkistan is limited to raw silk, carpets and felts, and a drug, known as Cheras to the Turkistanis, and as Hashish to the European. The fruits grown are those that thrive in a temperate clime, such as apples, pears and grapes; and melons grow to greater perfection there than elsewhere.

The Government of Turkistan is chiefly administered by the Chinese, whose practice of making all local officials responsible for insurrection, riot or disturbance in their neighbourhood, is peculiar, but entirely successful.

The towns and villages that lie buried under the sands of Turkistan have been kept in a wonderful state of preservation for close upon twenty centuries, and under the supervision of explorers and archæologists from Europe, excavations have been carried on, and highly interesting are the relics that have been brought to light. In what must have been an ancient graveyard the bodies of some well-preserved Chinamen were found, clothed in the picturesque garments of the Tang dynasty. No garments so ancient as these have been preserved by the Chinese in China Proper. Specimens of silk and fragments of embroidery have also been unearthed, and these afford valuable information on the condition of the textile industry in China ages back, and should be of enormous interest to the weavers and spinners of the present day. Manuscripts, many in forgotten and now unknown tongues, have been unearthed, and are a mystery save to a few famous scholars of Oriental languages. In addition to these records, which are on paper, birch, white leather, wooden tables and silken cloths, a number of frescoes, or wall paintings, from ancient Buddhist temples have been recovered, all things of great interest because they show some of the stepping-stones by which the Græco-Buddhist art of North-west India advanced across China to Japan. Our own Christianity is not unconcerned in these archæological discoveries, as there were once Nestorian Christians in Turkistan, and fragments in Syriac of the New Testament Bible have been yielded up by these sands, which can well become a "storehouse" for the preservation, in a desiccated form, of the remains of many bygone cultures. Turkistan stands alone in the possession of a climate sufficiently dry to preserve from decay the more perishable, but by far the most instructive, records of human activities and human thought. Who knows what pages of history—ancient, but as yet unrevealed—still lie hidden under the sands of Turkistan?

[The Lecture was illustrated by a series of lantern slides, showing the approaches into Turkistan along the British-Indian route to Yarkand and Kashgar, crossing the Himalayas diagonally, starting from Rawal Pindi, in the plain of the Punjab; the method and various ways of travelling over the heights and plains of the journey; the towns and townsmen met with on the way; the nomadic tribes and their self-made huts; the inhabitants of the plains at the skirt of the mountains of Turkistan; the ways of life of a people whose habits and character are subject to their physiographic surroundings; the many relics and shrines referred to in the Lecture.]

[G. M.]

WEEKLY EVENING MEETING.

Friday, May 16, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

S. F. HARMER, Sc.D. F.R.S. M.R.I., Director of the Natural
History Departments of the British Museum.

Subantarctic Whales and Whaling.*

[ABSTRACT.]

THE history of Whaling in Northern waters, and of the hunting of Sperm Whales in warmer seas, has often been written, and some of the principal facts relating to these subjects are matters of common knowledge. There is reason to believe that the existence of a whaling industry, which was inaugurated just outside the South Polar Circle after the commencement of the present century, is by no means generally known. Although Captain Cook, Sir James Ross and others had many years before reported the presence of whales in those latitudes, no practical advantage was taken of the information under fourteen years ago; and since that date the industry has eclipsed in importance all that had been done previously, even when the Greenland Whale "fishery" was at its height.

In 1892 Capt. C. A. Larsen left Norway for the far South, which he reached in the October of that year. No whaling was done, and an Expedition to the same regions, fitted out by Capt. Svend Foyn in the next year, was also unproductive of whales, mainly for the reason that it had been intended to hunt Right Whales and Sperm Whales. The Norwegian captains brought home, however, a very vivid impression of the enormous number of whales frequenting Subantarctic waters; and the fact that they at first made no further ventures was due to the profitable nature of the whaling in the neighbourhood of the Norwegian coasts. Dr. W. S. Bruce had simultaneously (1892) accompanied four vessels of the Dundee whaling fleet to the Antarctic, and had been similarly impressed with the abundance of whales in these waters. A meeting was shortly afterwards held in the rooms of the Royal Scottish Geographical Society at Edinburgh, to advocate the use of the modern Norwegian methods of whaling in the South; but the proposal was not carried, and no practical steps were taken.

Capt. Larsen subsequently became the Commander of the

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"Antarctic," the exploring vessel of Dr. O. Nordenskjöld's Swedish South Polar Expedition, 1901–1903. The "Antarctic" was wrecked; and Capt. Larsen, on his return journey, found himself at Buenos Aires, where in 1904 he founded the Compania Argentina de Pesca, the first Whaling Company which undertook operations in the far South. This Company commenced work at South Georgia in 1905, while the South Shetland Islands were visited, with the same object, in 1906, and the South Orkney Islands in 1911. The operations have proved so successful that there are now numerous companies whaling at South Georgia and the South Shetlands, a large proportion being Norwegian. While the most successful whalers in the Greenland industry, from the seventeenth to the nineteenth Centuries, were the British and the Dutch, the Norwegians have almost a monopoly of the art at the present time, and nearly all the skilled workers are of that nationality.

The older whalers hunted with hand-harpoons from small boats, provided with sails and oars, which were launched from the parent ship on sighting a whale. The objects of their chase were principally the Greenland Whale, the Atlantic Right Whale and the Sperm Whale; and they were unable to attack and capture the larger and swifter Rorquals.

About 1865 the Norwegian whaling Captain Svend Foyn invented the modern whaling-gun, which was fitted with an explosive tip and with a barbed harpoon carrying a strong rope. The explosion was regulated so as to occur immediately after the harpoon hit the whale, which is sometimes killed at once, and in any case is severely injured by a successful shot. The gun is carried in the bow of a steam-whaler, which chases the animal until a favourable opportunity for shooting occurs. These methods have revolutionised whaling, and there is now no whale which is too large to be captured.

In the prosperous days of the Greenland whale "fishery," 1437 whales were caught by 76 ships in 1814—an average of not quite twenty whales to each vessel—and this is mentioned by Scoresby (1820) as a specially good year. At the present day the number of whales caught by a single vessel, during the whaling season of six months, may rise to over 300; and the total number caught off South Georgia and the South Shetlands together has exceeded 10,000 in one year. Bearing in mind the universal history of whaling in the past—a period of prosperity succeeded by a rapid decline and a final abandonment of the industry—the question arises whether there is not a serious danger that Subantarctic whaling will have a similar experience.

The question of the disappearance of the whales is not merely a sentimental one, though Zoologists would naturally view their extermination with deep concern on scientific grounds. The plea for their preservation may be strengthened, however, by emphasising the fact that they are of the highest economic importance. The baleen or whalebone of the Right Whales is a material of much practical utility for many purposes; but its importance is almost negligible compared with that of the oil which is derived from the blubber and other parts of whales. Whale-oil can be readily transformed into soap and glycerine, while it is possible to prepare from it a fat which is perfectly inodorous and is utilised in the manufacture of margarine. During the War it has been of vital importance. Enormous quantities of glycerine derived from it have been used for the manufacture of explosives, and it has been hardly less important in its relation to the food-supply. If whale-oil had not been obtainable, the glycerine which was essential for our National security must have been derived from other animal fats or from vegetable oils; and the shortage of fat required as food would have been very serious. After the oil has been extracted on the whaling grounds, the remainder of the carcass may be dried and ground down into "guano," which is valuable as a fertiliser for crops and is also utilised for the preparation of cattle-foods. It is not always possible to carry out this part of the process; and an enormous waste of valuable material may result from this omission. Since it is well known that the flesh of Cetacea is fit for human food, it is by no means impossible that a part of the enormous quantity of meat which might be obtained from them may be so utilised in the future.

Although a few Right Whales and Sperm Whales are captured by the Subantarctic whalers, who occasionally kill some of the smaller Cetacea as well, the industry in these waters is almost entirely confined to three species of the larger whales. Of these the Humpback rarely exceeds 55 feet in length, the Fin Whale is not much more than 85 feet, while the Blue Whale, probably the largest animal that has ever existed, is sometimes over 100 feet long. In the first few years of Subantarctic whaling, the Humpback constituted nearly the whole catch, even more than 96 per cent in 1910–11, when 5299 individuals of this species were captured off South Georgia in the six months of the principal whaling season. In 1912–13 the number of Humpbacks caught in the same locality, in the corresponding six months, fell to 2251 (about 53 per cent of the total catch), and in 1913–14 it was reduced to 474 (about 18 per cent). This diminution, which has persisted to the present time, has been due largely to a reduction of the number of Humpbacks frequenting South Georgia; but it has been partly caused by an increase in the size of the whaling vessels and of the strength of the tackle employed, enabling the whalers to hunt the larger kinds, which naturally yield more oil and other products than the comparatively small Humpback. The whaling industry thus depends at present almost entirely on the Fin Whale and the Blue Whale. It is noteworthy that the Fin Whale, of intermediate size, first rose to prominence on the decline of the Humpback; but the gigantic Blue Whale has now surpassed it and has become the favourite object of the whalers' pursuit.

There is at present no certain evidence of the reduction in numbers of Fin Whales and Blue Whales.

In explaining the reduced number of Humpbacks frequenting the whaling grounds, the whalers rely on the hypothesis that individuals of this species are of a timid nature, and are readily frightened away from a locality by pursuit. There is probably some truth in this view, but it is at least possible that the reduction is due to a diminution in number of the total stock of Humpbacks. Whales are migratory animals, and their movements are almost certainly influenced by two causes: (1) the distribution of their food-supply; (2) the position of their breeding grounds. The Plankton-organisms on which the Whalebone Whales subsist are present in vast quantities in Polar waters during the summer, and the whales are accordingly found there at this period. Towards the end of the summer or the beginning of autumn most of them forsake high latitudes. The Southern Humpback executes extensive migrations northwards, along the coasts of the great Southern Continents, to the neighbourhood of the Equator and even beyond it. It is in these warmer waters that it is known to breed, and it subsequently proceeds southwards in the ensuing spring. One of the most alarming facts about this species is that it has been extensively hunted along the coasts of Africa, South America and elsewhere. Although it is not possible to assert positively that the South Georgia Humpbacks are thus affected, there are strong reasons for believing that this is the case; and it would thus follow that this species is persecuted in Subantarctic waters during the summer, and further north during other parts of the year. Remembering that the old whalers reduced the Greenland Whale almost to the point of extermination by the use of what may now be regarded as primitive methods, and that a similar fate has befallen the once flourishing whaling industries of other localities, it thus appears that there are the most urgent reasons for seeking to afford some immediate measure of protection to this and other species of whales.

In devising methods for the protection of animals, the principle of saving them from being hunted during their breeding season has been found specially effective. It is very difficult to get complete information on this subject with regard to whales, but one of the ways in which a conclusion may be reached is the examination of fœtal records, a method which has already been adopted with some success by Guldberg and others. By the study of a relatively large mass of statistics which has been supplied to the British Museum (Natural History) by the Whaling Companies operating off South Georgia, I have found it possible to arrive at certain definite results which amplify or correct those of previous observers. These are specially clear in the case of the Fin Whale, which has provided the largest number of records. It is found that in a given month there is a particular length of fœtus which has the greatest frequency;

that in the next month a greater length is most common; and so on for several of the succeeding months, after which the whaling season comes practically to an end, and the number of records is inadequate to give a normal result. Although there are certain irregularities needing explanation in the graphs constructed from the statistics, the general result has been arrived at that, in each of the three species principally hunted at South Georgia, pairing takes place with greatest frequency at a certain period of the year, and that a normal curve of pairing can be drawn. This result gives in the main a satisfactory explanation of the statistical records. The season when pairing is at its height falls, in each case, outside the period when whaling is actively carried on in the far South; and the important conclusion is reached that if the whales are to be protected during their breeding season, it must probably be done in regions of the world further North than South Georgia. The validity of the Southern figures, which have no doubt been roughly recorded by the whalers, has been confirmed by obtaining corresponding results from the examination of the statistical fœtal records of Northern whales.

It can hardly be doubted that protective measures of some kind are urgently necessary now, or will at least become so in the near future; although it is by no means certain what form they should take. The British Government is in some respects in a specially favourable position with regard to this matter, since all the important whaling grounds of the Subantarctic region belong to the Dependencies of the Falkland Islands and lie in its jurisdiction. It is satisfactory to be able to conclude with the statement that the Government is fully alive to the necessity of taking steps before it is too late, and that an Interdepartmental Committee is at present engaged, under the auspices of the Colonial Office, in framing a scheme for an Expedition which is to investigate the whaling problem on the spot, with the view of obtaining information on which legislation may be based.

[S. F. H.]

WEEKLY EVENING MEETING,

Friday, May 23, 1919.

Dr. Henry E. Armstrong, LL.D. F.R.S., Vice-President,
in the Chair.

Sir Alexander C. Mackenzie, Mus.Doc. D.C.L. LL.D. M.R.I.,
Principal of the Royal Academy of Music.

Hubert Hastings Parry:
His Work and Place among British Composers.

Before saying more, let me gratefully acknowledge the fact that this famous Institution, primarily devoted to the advancement of Science, has at no time in its history overlooked or neglected that of the Arts. We owe particular thanks, because it has during a long period of years shown a most generous and very practical appreciation of music by extending to its prominent representatives the welcome privilege of speaking here. It is just one hundred and fifteen years since Dr. Crotch was invited to give the first course of lectures on music, and without attempting to enumerate all the links in the long chain, I will only add that the honour of delivering the first Friday Discourse fell (in 1866) to my predecessor, Sir George Macfarren.

Sir Hubert Parry, who frequently lectured in this room, ended his one and only Discourse (1890), "Evolution in Music" being the subject, with these words:

"There still are martyrs who sacrifice their lives to ideals, and look for neither popularity nor pay; and it is still possible in music to write what the present generation will take no notice of, but the next will cherish."

And I can find no better or more apt a starting-point than that significant sentence.

It has been urged that it is not for those of his own generation to attempt adjudgment of the work, or to speculate upon its future place, of so outstanding a figure in contemporary art as that of Hubert Parry.

Although approached with diffidence, I am not sufficiently timid to forbear the endeavour.

To speak of a so recently lost comrade would have proved infinitely more trying, had not the preparation of my theme necessitated a re-reading of the familiar scores; and so strongly stamped is his likeness and character upon them all, that any misgivings—and I confess to have experienced some—concerning the difficulty of treating them impartially or dispassionately were dispelled as if by an encouraging and unbroken personal contact.

It is by no means my intention to be biographical—that kind of information is easily obtainable—but the case is singular: even freakish.

That Art can never have been, even in his earliest days, a mere pastime or pleasant accomplishment is evident; and that the eminent master to be was one of Nature's own selection is equally certain. To secure the degree of Bachelor of Music at eighteen presupposes an amount of most earnest devotion and serious application surely rare in the history of the leisured human boy!

Oddly enough we have a strikingly identical instance of precocity and industry in the person of a certain William Crotch, who obtained the like distinction (in 1794) at the same age. Parry was composing at Eton at fourteen. Crotch produced an oratorio at Cambridge at fourteen.

Parry succeeded to the Directorship of the Royal College of Music; Crotch was the first Principal of the R.A.M.

Nor does analogy quite end here; Parry, inheriting his father's artistic tastes, occupied himself at one time seriously with drawing; while Crotch became a skilled and well-known painter in water-colours.

There was, however, one happily short period in early manhood when the absorbing passion for music had perforce to be subdued in favour of a decidedly uncongenial occupation. And when he once, with a hearty laugh, alluded to his losses at Lloyd's as "the very best thing that ever happened to me," I cordially agreed, and added "and to us."

The unskilled "'prentice hand" is hardly traceable at all, even in those many early examples of chamber music, then known only to an intimate circle of friends. But technically ripe and finished as these are, his natural buoyancy of spirits and habitually cheerful outlook on life is more clearly revealed in the songs and other less elaborate pieces composed about the same time.

It was not until later, when a pianoforte concerto was played at the Crystal Palace—where not a few of us were baptized about the same period—and "Prometheus Unbound" was produced at Gloucester in 1880, that the general public was faced and the musician's career began in real earnest. Looking back over that stretch of time we realise more clearly the reasons why the undoubtedly great merits of the cantata received no more than partial recognition. The interminable Wagner disputation was then at boiling-point. The German master's latter manner was yet far from proving acceptable to all, even in his own country; much less so in ours, where a newly-awakened interest was just beginning to encourage native effort. The Wagner bewitchment is perceptible enough in Parry's poetically conceived work; and that a young British composer should express himself in so debatable a style was perhaps resented. But just those sections, in the second part, which foretell the coming of the Parry we now know, were selected for praise by the more discriminating. Maybe this failure was another providential "happening"; for, following the sequence of events, may we not assume that "Prometheus Unbound" Parry, released him from some early stifling influences, and caused him to find himself with characteristic instinct and energy? The conjecture is confirmed by the subsequent rapid development of that noble musical speech which remained peculiarly his own.

The "Glories of our Blood and State" was the precursor of a work exhibiting every quality which goes to the making of a masterpiece, for the Miltonic grandeur of "Blest Pair of Syrens" is not likely to be surpassed. Here indeed is "immortal verse" wedded to music which none but an English musician of highest attainments could have produced. And the same purely native spirit pervades the settings of some of the choicest examples of our literature, such as Pope's "St. Cecilia's Day" and Milton's "L'Allegro ed il Penserioso." Some composers think in orchestral, others in pianistic terms, according to the promptings and chances of their early training.

If one may so put it, Parry thought "chorally"—loving big strokes on large canvases—and these fitting vehicles for his gifts, with their descriptive episodes and picturesquely varying moods, afforded that spaciousness which his chosen medium demanded.

To quote his own printed words, "There is hardly any nation worse supplied with music which represents its true characteristics than the English," and the elevation of British choral music became his quest and mission.

This idiomatic consistence is nowhere more apparent than in his only two oratorios of full dimensions. We admire the pictures of the old masters none the less because Biblical personages are represented in the costumes of the period and the country in which they were painted.

Similarly, there is something almost quaintly characteristic about "Judith," and the even more dramatically-inspired "King Saul," in which no attempt is made to tinge the old Eastern stories with so-called "local colour."

Judith is a British heroine, and Meshullameth's delightful children—saved from the priests of Moloch—are Gloucester-born and bred.

I say this with all admiration for a work of which I had the honour of conducting the first two performances in London (1888), when I never quite succeeded in taking the *tempi* fast enough to satisfy the impulsive composer. That perhaps was the Welsh part of him!

Let not that forceful dramatic instinct, at times even vehemently expressed, be under-estimated or overlooked, as it frequently is, because of the Classicists' horror of the theatric and superficial, which so effectively checks the faintest approach to sensationalism or mawkishness.

In the well-known long lamentation Job does not wring his hands, but takes his afflictions and sufferings like a man: and a duet between Judith and Holofernes from that pen is simply unthinkable!

As it is perhaps possible to nurse these sterner tastes with too great a solicitude, I may allow myself further reference to them presently. It is the self-imposed reticence, the uniformly-maintained manliness of style, which impresses all the more by the absence of austerity.

To play, polyphonically, with huge choral masses at will, as upon an instrument, with such consummate ease, argues the acquirement of transcendent craftmanship. There was something more than that.

Always receptive, and observant of every inch of real progress, he persistently broke fresh ground in a field for centuries particularly our own; and pointed the way to some of these newer present-day experiments in choral treatment which promise to keep us still in possession of it.

But the Arts-man Parry presents several distinctly separable and very personal temperamental qualities.

He of the boldly energetic and majestic measures, of the broadly sweeping melodies, alternates with the prolific writer of these many compact little tunes which run like delicate veins in blocks of marble, and touch us by their ingratiating simplicity.

To fully appreciate that gentler side we have but to look at the ten sets of English Lyrics, to which he seems to have turned as if to seek relief after each more strenuous effort; for the series starts early and ends late in the long catalogue.

It would have been a miracle had wit and good-humour been to seek in the music of one possessing so large a share of it.

Hear "Laughter holding both his sides" in the Greek Comedies: and "Honour due paid to mirth" in the droll treatment of the "Pied Piper"—now an accepted classic.

Our time-limit forbids even shortest discussion of the manner in which the satiric humours of the Comedies are illuminated by apposite quotations from every conceivable source. These whimsical comments, indirectly, also throw light upon some of his own personal mislikes.

In the "Frogs," for instance, Gounod and a special *bête noir*, Meyerbeer, figure prominently.

And in the much more elaborate "Clouds," the ludicrous combination of Beethoven, Wagner, Tchaikovsky, Strauss and others with popular and comic songs makes that most excellent fooling which only wise men may safely indulge in.

Here is our old friend the "Perfect Cure" dovetailed in a Bach Fugue, passing through "Rule Britannia" into Sullivan's "Lost Chord." There is method in the madness, for the words run:

> "Though you make a bad beginning, still you somehow
> muddle through:
> And from e'en your latest error read how good may
> come to you." *

Now, while all this splendid nonsense contains nothing set down in malice, the gay irresponsibility also serves to deal out an occasional hard knock at some prevailing musical tendencies, cleverly imitated and, so to speak, "nailed to the counter"; as if to say, "I can be just as crazy as anybody, when I like."

Dodgson or Calverley have done nothing better.

Come we now to that purposeful sequence of what may only be called "appeals" for brotherhood among the peoples, in which both poet and musician speak and sing so earnestly, and the idealist urges the belief that highest happiness is to be found in the individual endeavour—for others' sakes.

That the life that is worth living is a dedication; that the spirit should be girt for whatever may befall, is practically his own summing up of the meaning of the Fourth Symphony (revised in 1910).

"Finding the Way" is its significant motto. And its detached movements are headed "Looking for it," "Thinking about it." "Playing on it," and finally "Girt for it."

The more comprehensive title "Life" would have fitted that work, as well as the Fifth Symphony of 1912—so far as I am aware his last and most effective effort in purely orchestral music. In the latter the same intention is predominant, and evidently not to be dismissed from the composer's mind.

The linked movements are thus sub-divided: "Stress," indicating revolt against the tragedy of life, bringing in its train suffering and distress. Then "Love," in which lies true hope of healing, human love calling and answering. Thirdly, "Play," also having its genuine province, and the inexhaustible instinct of humanity for fun and humour its share in helping. The last movement is

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* The "Acharnians," written as late as 1914—not many months before the declaration of hostilities—is now of peculiar interest.

In the final bars you will hear, simultaneously, scraps of the "Meistersinger," the "Marseillaise," an old comic song called "An Norrible Tale I have to tell," and, as the curtain descends, "Die Wacht am Rhein" with our "British Grenadiers" on the top of it.

Coming events did certainly cast their shadows before.

"Now!" and depicts hopefulness rudely broken in upon and extinguished by tragedy, which in its turn becomes, in the light of human love, the token of healing.

The text of it all is best delivered in his own words, "What is Love? The one thing that availeth. What is our hope? That good through Love prevaileth."

This doctrine is taught continuously in a chain of choral cantatas, commencing in 1903 with "Voces Clamantium," and completely expressed in an elevated poem, "The Vision of Life," which reads and sounds as if word and tone had been created simultaneously. But for the war this mature and convincing proof of his genius would have been performed, in its expanded and final form, in 1914. We yet await that important event.

The same spiritual idea, the call to humanity, pervades all the later productions, none of which were penned without that underlying motive.

The precious gift of his last years to our organists—namely, the fourteen Preludes and the three great Fantasias on Hymn-tunes—conceived in the monumental manner of his beloved Bach, is another evidence of this pious fervency.

Whether an inner premonition prompted into being the group of six motetts called "Songs of Farewell" so near the end we may not venture to guess. In that connection it is enough to possess in these, to us, pathetic pieces some of the most sincerely expressive and perfect specimens of English musicianship.

Believing in the justice of posterity, I venture to think that it is not premature for those who understand him to indicate what the ultimate position of so complex an appearance among us as Parry the composer, poet, thinker, historian, teacher and idealist should, or rather must be; and even after this necessarily incomplete and obviously sketchy survey, no doubt you are as able as I am to foretell its exceptional prominence.

The life-work of one who, instead of bidding for a more easily gained popularity, preferred rather to stand aside from the crowd must needs wait—maybe wait long—for that full recognition which its very steadfastness of purpose is bound to secure for it.

It is one thing to have won a distinguished name, quite another to be rightly comprehended, and I may safely say that Parry is as yet known and appreciated only by the comparatively few.

May we not therefore at a time when native creative art, with all its promise of a great future, is in a state of flux and running into all sorts of eccentric moulds (by no means of our own fashioning), conveniently and profitably turn into that splendid library of essentially British music left to us by a master?

There are limitations, no doubt. While his critical writings abound in well-reasoned and apt analogies between the two sister-Arts, it would be easy to name some who are provided with richer palettes and can use orchestral colour with defter hands. I say "orchestral" advisedly, because the gift of painting the "word" unerringly by means of punctiliously chosen harmonies, accurate inflexion and syllabic accentuation, droop and rise of the phrase, are precisely among the secrets to which he possessed the key.

So, too, one might point to composers of more pronounced emotional warmth—not of necessity mere limp sentimentalists, or something baser on that account—of wider range and lighter touch.

Although their natural temperamental bents tend in totally different directions, their convictions are equally true and genuine, and it would indeed be a dull musical world without them.

The habit of analysing and philosophising too much about every new manifestation in art is perhaps more apt to confuse and obfuscate than help; and in less self-possessed men may cast a chill "All Winter and severe" over such imagination and inspiration wherewith they may happen to be endowed.

But between the lines of Parry's latest published staves we may perhaps read something importantly significant.

A violent abuse of glaring pretentious effect, like splashing paint about with a mop out of a bucket; an increasing encroachment of noisy realism, shrieked out by an over-grown monster orchestra: and the degeneration of honest emotion into the hysterically morbid — and worse — were among other much too readily accepted but thoroughly unwelcome importations from certain quarters for some years prior to the war.

But however unlikely to affect, much less to taint, the lofty aims of one who believed that "the music which is going to live longest is that which addresses itself only to the highest mental faculties," he appears—if deductions from dates and events are fairly correct—to have receded more and more within himself, the better to avoid contact with probably the ugliest and most destructive tendencies known in the whole history of the art.

I hope we are now better able to appraise it at its real worth.

Living as we are, however grudgingly conceded, in one of the most active and liveliest musical centres in Europe—one which promises to be, now more than ever, *the* great cosmopolitan arena open to all comers.

And exceptionally well provided as we are, and always love to be, with willingly seized opportunities of acquiring an almost immediate knowledge of the very last word in the music of every nation—except perhaps our own—it demands a rare power of concentration, or, better said, of absolutely water-tight detachment, to enable even the resolution of a Parry to keep unswervingly to the straight independent course he mapped out for his own, and so honourably ran to the end.

Therein lies the kernel of a noteworthy example, the drift of a great lesson; and in that respect I claim him—using the word in no narrow, but in its most liberal sense—as a foremost "Nationalist" in music.

I have tried to show what Parry actually did achieve—a magnificent record—and refrained from speculating upon "probabilities" and "possibilities"; because I cannot bring myself to think that the sacrifices of time and energy he chose to make are all to be accounted as losses to Art.

Parry had the power of radiation, of inspiring enthusiasm in others, and delighted in exercising it. And it is something to have had such a man personally touching at least a couple of generations of young musicians on the shoulders in the great school of music he loved so much.

Sufficient to know that he leaves us with no regretful sense of an unfinished life-labour. With William Watson we may say:

> "No record Art keeps of her travail or throes.
> On the steeps there is toil, on the summit repose."

In his last utterances you will perceive a calming-down of the characteristic impetuosity; a mellow serenity, approaching almost to resignation; but never a pessimistic, dissatisfied note, "jarring against Nature's chime." On the contrary, the hopeful, courageously cheerful, open-air man, by instinct and breeding, is speaking to us and to our successors through his music all the time.

[A. C. M.]

[Musical illustrations were furnished by Miss Agnes Nicholls, Mr. Hamilton Harty, Mr. Arthur L. Sandford, and a number of Students from the Royal Academy of Music.]

WEEKLY EVENING MEETING,

Friday, May 30, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

SIR JOHN ROSE BRADFORD, K.C.M.G. C.B. M.D. D.Sc. F.R.S.,
Late Consulting Physician to the British Expeditionary Force.

A Filter-passing Virus in Certain Diseases.

THE term virus is applied by pathologists to a living agent that is proved to be the actual cause of any given disease, and the progress of knowledge tends slowly but surely ever to increase the number of diseases in which such a definite cause is established as the essential factor in their production. In many, perhaps in all, diseases more than one factor is necessary, thus so-called predisposing causes may not only always be necessary, but may sometimes be of more immediate practical importance than the presence of the actual virus. Measures directed to control the ill-effects of predisposing causes may be of more avail in controlling disease than measures directed to destroying the actual cause or virus. Nevertheless, the study of the actual cause of disease remains of paramount interest and importance, inasmuch as the satisfactory study, and therefore the satisfactory control, of disease is impossible until we know its cause with certainty. Unless we can diagnose disease with absolute certainty, and not merely as a matter of opinion, the position is unsatisfactory. In many instances diagnosis in the real and strict sense of the word is impossible when the cause of the malady is unknown. With the discovery of the cause, methods of diagnosis of extreme accuracy can frequently be devised, and then a malady can be recognised with certainty. Further, such discoveries often lead to the recognition of other facts of importance, and slighter forms of the disease in question, previously either not recognised at all, or else mistakenly regarded as belonging to some other group, are detected. This is often a point of great practical importance, as is also the somewhat similar fact of the discovery of what are known as "carriers"—i.e. individuals not themselves ill, but nevertheless harbouring and capable of conveying to others the infection, it may be, of some virulent and dangerous disease. All these facts depend upon the discovery and recognition of the specific virus or cause of the disease in question, and remain unknown until this specific virus is isolated and proved to be the true cause of the particular malady. Discoveries and facts of this nature have often explained the mystery of the apparent spontaneous origin of diseases at certain places at certain times, when there is no evidence of importation of actual cases.

Sufficient has probably been said to emphasize the importance, if medicine is to be truly scientific in its methods, for the causes of disease to be studied as fully as possible and placed on a sure foundation. Great progress in this direction has taken place during the last seventy years, and especially during the last fifty years; but modern investigators, with all the means at present available at their disposal, cannot fail to be profoundly impressed with the foreshadowing of results often arrived at by the fathers of medicine.

Speaking broadly, the great majority of diseases are due to the direct action of one or more of the following causes: (1) physical agents; (2) chemical agents; (3) living agents, or viruses. In the past much stress was laid on the first group, as, for instance, the influence of cold as a direct cause of disease. Even as recently as thirty years ago exposure to cold was regarded as the main cause of a great number of diseases of various organs of the body, such as the lungs, heart, kidneys, spinal cord, etc., and not only so, but the a-nomenclature was introduced, and still obtains, suggesting that these affections were actual inflammations, due probably to extreme congestion of the internal parts produced by the direct action of the external cold. Modern investigation has shown that in most of these diseases a living virus was the effective cause, and that cold, if it had any action at all, only acted as a predisposing cause. Hence, at the present day, it is only in a very limited class of diseases that we recognise physical agents such as heat, cold, changes of pressure, as true effective causes.

There can be no question that some diseases are due to chemical agencies, and here maladies may be produced in one of two ways: (i.) by the direct action of some poisonous material; or (ii.) so-called deficiency diseases, where pathological changes result from the withholding of some substance essential to the wellbeing of the body.

Most diseases of man and animals where the cause is known, are dependent upon the action of a living virus, and thus the study of disease is really a branch of Biology, and strictly a phenomenon of Parasitism. The known pathogenic viruses belong some to the vegetable kingdom, some to the animal, and some are not as yet sufficiently known to make their classification at all certain. Some of the most important diseases, especially perhaps those of tropical regions, are due to animal parasites either protozoal or of the higher phyla of the animal world. The diseases of vegetable origin are mostly due to bacteria; but yeasts and fungi are also factors of some importance.

The bacteria, or micro-organisms, are very numerous, responsible for very many diseases, and the greatest triumphs in medical science of the last fifty years are the results of the discoveries of Pasteur in this field.

Reference was made to the important works of Leuwenhoek in 1683, Davaine in 1850, Koch in 1876, and Pfeiffer in 1892.

In the last twenty years several diseases have been shown to be due to quite large organisms only revealed by special methods of staining or by special methods of microscopic technique. The organism responsible for syphilis is a striking example of this, and now with improved methods there is no insuperable difficulty in demonstrating it when desired. Japanese observers have by somewhat similar methods succeeded in clearing up the causation of a hitherto obscure form of febrile jaundice, and shown that it is due to the presence of a spirochæte, an organism of very appreciable size, and this Japanese work has been fully confirmed by other work carried out on this disease amongst the troops in the B.E.F.

There is, however, evidence available in support of the view that some organisms are so extremely small as to be either beyond the range of vision, or at any rate extremely minute, and Pasteur himself suggested that future work might show that some organisms were exceedingly small. This evidence is afforded by the results of the use of filters in bacteriological work. Organisms during their growth produce, like all living things, a number of chemical products as a result of their vital activity. These substances are often very violent poisons, and many of the symptoms of disease are due to the direct action of these poisons on the tissues and organs of the body. In order to study the processes of disease and of cure it is essential to separate the organisms from their products and to study separately the action of either the organisms, or of their products on the experimental animal. Some of the most valuable advances in modern medicine, and more especially in the treatment of disease, have originated from discoveries made in this line of work, and it will suffice to mention the treatment of diphtheria and that of tetanus as illustrations. In both of these an essential factor underlying the work was the separation of the poisons or toxins from the organisms by the use of filters. Such filters must be of such a nature that the pores of the filter are extremely small, as it is essential for success that the organisms should not pass through. Such filters are usually made either of siliceous earth or else of unglazed porcelain, and many filters of different porosity, and usually in the form of hollow candles, are on the market; they differ greatly in their efficiency and always require careful testing. It is usually necessary to employ pressure in order to filter, and this may be applied either as a positive pressure to the original fluid or preferably by aspiration so as to suck the fluid through. The use of such filters must be carefully controlled, and there are many sources of error and possible fallacies, such as imperfections in the filter, the employment of excessive differences of pressure, or too long a time being spent in the filtration, so that an organism has sufficient time to actually grow through the filter. In the hands of careful and experienced workers, and with frequent suitable controls and tests, the use of such filters is most valuable, and in fact indispensable to such work as at present concerns us. These filters were used in the first instance, and are still used, for the purpose of separating organisms from toxins, and the filtrates obtained were sterile, toxic, but not infective, in their action.

In the year 1898 a remarkable discovery was made by Löffler and Frosch. These observers were studying foot-and-mouth disease, a contagious malady affecting both man and cattle, and were filtering lymph from infected animals on the usual lines with the object of obtaining a toxic but germ-free lymph. They found, however, that a minute quantity of the filtrate, i.e. 0·02 c.c., was still capable of producing the infection, although they were unable either to see or to culture any organism from the filtrate. This was the real origin of the belief in filtrable or filter-passing viruses, i.e. living organisms so small that they would pass through the minute pores of a porcelain filter that held back all the hitherto known varieties of micro-organisms. It was also supposed that such forms might be ultra-microscopic, but doubt was soon thrown on this latter hypothesis, as Roux and Nocard in the same year, 1898, succeeded by a special method in showing that the filter-passing organism of pleuro-pneumonia was visible as a minute dot under a magnification of 2000 diameters. It was thus clear that certain organisms were entitled to be called filtrable or filter-passing, but no confirmation of the theory of their being ultra-microscopic was obtained.

From this time onward additions to knowledge were made, so that at the present time a considerable number of human and animal diseases are regarded as due to filtrable viruses, but it will suffice here to draw attention to two discoveries of importance. In 1903 Dorset and McBryde produced evidence to show that swine fever, the ætiology of which was very obscure, was really due to a filtrable virus, and that the bacillus to which it had been attributed before was really an associated organism. This observation was of very considerable importance, in that it suggested the possibility that some diseases where there was much confusion and discordance were really due to the action of two viruses, a filtrable virus as the primary and essential cause, and then a second constantly associated organism of a different type, e.g. a bacillus. This is an important result that may have applications to some human diseases of doubtful causation. The other discovery was also an extremely important one, having a very direct bearing on the matter before us, i.e. the elaboration of a method by which a virus capable of passing through certain filters was successfully grown and rendered clearly visible by staining. This was a great step in advance, as it removed some of the mystery that up to this time surrounded the filtrable viruses, and showed that in many respects these viruses were analogous to other better known and larger organisms. There is a well-known malady, poliomyelitis, sometimes called infantile paralysis from the frequency with which it attacks children, that is always present amongst us to some degree, and which sometimes assumes epidemic proportions in various parts of the world. Although many of its features were well known, its cause was unknown until Landsteiner and Popper in 1908 showed that the disease could be transmitted to animals by inoculation with material derived from the spinal cord of fatal human cases. This was confirmed by Flexner and Lewis in 1909. At this time it was also known that the virus was filtrable through certain filters, but the great step forward was made in 1913 when, thanks to a method of culture devised by Noguchi, Flexner and Noguchi succeeded in growing the virus outside the body in the laboratory. This method is now known as the Noguchi method, and is the one that has been used in our work in France.

Flexner and Noguchi succeeded in demonstrating that the virus of poliomyelitis consisted of minute rounded bodies $0 \cdot 15 \mu$ to $0 \cdot 3 \mu$ in diameter, that it could be stained, and that the pure culture when inoculated into animals reproduced the clinical picture and the lesions of the original disease, and finally that the organism could be recovered from the tissues of such experimental animals. These observations not only showed that Koch's postulates had been fulfilled as regards this important disease of the nervous system, but they also showed that a certain filter-passing organism could be grown artificially, stained, and so rendered clearly visible under high powers of the microscope. Up to this time such organisms had only been seen as minute dots, but now it was possible to proceed further, and the virus in question was described as a globoid, not only on account of its rounded appearance, but also because staining revealed some differentiation into a more or less central darkly staining portion surrounded by a zone of variable width of less deeply stained material. Further, they tend to form masses or colonies, having a zoogl&oelig;a-like appearance, in that it is difficult to separate sharply the contours of the individual elements. The darkly stained central portions of the organisms stand out conspicuously in the mass of more faintly stained material. The position of these globoid bodies in the scale of nature was and is still uncertain, although from their culture, and also to some extent from their staining reactions, they show affinities to the bacteria and thus to the vegetable kingdom.

Another American observer, Foster, in 1917 studied, with the aid of the Noguchi method, that familiar ailment the common cold, and was successful in growing a minute filter-passing organism in this disease.

We have now concluded a short and necessarily imperfect summary of the principal results obtained in this branch of knowledge prior to the war, and I will now pass on to the work carried out during the last two years in the British Expeditionary Force in France in certain field laboratories attached to No. 20 and No. 26 General Hospitals. This work was carried on with the co-operation of Captain J. A. Wilson and Captain E. F. Bashford, and the results obtained are mainly dependent upon their work. This work did not arise as an investigation of so-called "filter-passers," it began as an enquiry into the causation of a peculiar and little-known form of paralysis that occurred from time to time as a rare form of disease amongst the troops, known technically as "febrile polyneuritis." It will suffice to say here that up to the time of the war its causation was unknown, and it had been but little studied owing to its rarity. Observations carried out at Etaples showed, first of all, that the disease could be communicated to animals from nervous material obtained from fatal human cases, by a method similar to that used for the experimental transmission of rabies and of poliomyelitis. A small portion of nervous material is preserved for a short time in glycerine, and this emulsion inoculated intracranially and subdurally into the monkey. The disease can be transmitted in this manner to the monkey with an incubation period of six weeks, and the same lesions are found in the monkey as in man. These experiments proved that this so-called polyneuritis was fundamentally different to many other varieties of neuritis, in that it was really an infection, and thus had obvious affinities with poliomyelitis. Hence it was natural to try by further experiments whether methods of culture that had been successful in the case of poliomyelitis would be equally successful in polyneuritis. This proved to be the case, and Captain Bashford demonstrated the experimental transmission of the disease and Captain Wilson was successful in growing a globoid organism both from man naturally infected and from the monkey experimentally inoculated with material derived from fatal human cases. Further, the disease was also produced by inoculation of the pure subculture, thus Koch's postulates were complied with in regard to this malady.

The main point of interest in these observations was the discovery that the organism of poliomyelitis was not a unique and solitary example, since we had obtained a similar organism, differing in some respects it is true, from another, but allied disease. This became the starting-point of observations on a series of diseases, and the two conditions first investigated were rabies and trench fever. Rabies was selected because the clinical resemblance between the palsy of polyneuritis and that of dumb rabies gave us the first clue to the successful investigation of polyneuritis by experimental methods. Rabies can be communicated by this method of inoculation of glycerine emulsion of the brain, and polyneuritis was found to be so communicable also. Trench fever was investigated because of its great importance from a military point of view and because recent work had shown that the unknown virus was a "filter-passer." In both rabies and trench fever positive results were rapidly obtained, and minute filter-passing viruses successfully grown in the case of rabies from the brain of rabid animals, and in the case of trench fever from the blood of cases of the disease in man during the febrile period. They were also grown from the dejecta of lice that had been infected from human cases in England.

Positive results had now been obtained by this method from three widely different diseases of men and animals—polyneuritis, rabies and trench fever. The organism of the first disease was a globoid, but it soon became evident that the other two organisms were not globoids, as they showed no differentiation into two zones, and they did not form zoogl[œ]a-like masses; further, the organism of rabies was extremely minute, although extraordinarily sharply defined. At this stage of the work it seemed possible that the method was one generally applicable for the investigation of filter-passing organisms, since the virus both of rabies and of trench fever belonged definitely to this group, and therefore the scope of our enquiry was still farther widened. Influenza and a number of the acute specific fevers such as mumps, measles, typhus, etc., were investigated. These are maladies the causation of which is either unknown, uncertain, or reputed to be due to filter-passers, and thus it was natural to investigate them. Two other maladies in which we obtained striking results remain to be mentioned, encephalitis and nephritis, and it is necessary to say a few words as to the reasons for their investigation. Encephalitis occurred to some extent in England last year, although at first there seems to have been some uncertainty as to its nature. We did not see this malady amongst the British troops in France to any appreciable extent until this year, and personally I saw no cases of lethargic encephalitis in France until the spring of 1919. So soon as the polyneuritis results were definite, we obtained material from fatal cases of encephalitis in England, and a filter-passing virus was successfully grown from this material, and then when cases occurred in the British Expeditionary Force similar results were obtained from them. It was natural to investigate encephalitis, as it is a malady possessing affinities to poliomyelitis and to polyneuritis, and we have succeeded in obtaining results establishing the Koch postulates with reference to this disease.

As regards nephritis, there was no cogent scientific reason for trying the Noguchi technique, except that all known methods had failed in achieving any successful result in this malady, and there were, in the opinion of many observers, reasons for looking upon nephritis as due to an infection of some kind. Nephritis was known to occur as a complication in certain acute infections, but in a large proportion of cases of nephritis no evidence of an antecedent infection can be obtained, and the causation of the disease was quite obscure, and many had still a lingering belief that exposure to cold, and especially to damp cold, was the main ætiological factor.

The results of the work carried out at Etaples may be summarized as follows. Filter-passing organisms have been isolated by culture from the blood and from the secretions of the body in a considerable number of diseases of obscure origin, and these organisms fall into two definite groups. The first and smaller group consists of those entitled to be called globoids, and the second and much more numerous group is that of the true filter-passers. The so-called globoids are characterized especially by the fact that on staining they show a distinct differentiation into a central darkly staining and a peripheral less stainable zone. The latter has often a somewhat indefinite outline, so that it is difficult to separate with sharpness one individual from its neighbours. The non-globoid filter-passers are, on the other hand, extraordinarily sharply defined, although often extremely minute. The different members of this series present individual morphological differences not only of size, but also of shape and arrangement. Thus some are rounded, others oval; some form short chains of three or four individuals, others are arranged in pairs; others form a mosaic-like pattern. They also present differences in their culture—not only differences in the ease or difficulty of growth in the Noguchi medium, but also differences in the rate and mode of growth.

The organism isolated in rabies is the smallest we have as yet seen; it is in the form of a minute very sharply defined rounded body, varying in size from 0·1 μ to 0·3 μ in diameter. The organism of influenza is also very small, measuring 0·15 μ to 0·5 μ; it also has a very definite outline, and is a minute rounded or oval coccus-like body. The organisms of trench fever and of nephritis are distinctly larger, the former varying from 0·3 μ to 0·5 μ and the latter from 0·3 μ to 0·6 μ. Speaking roughly, they are two to three times the size of those found in rabies or influenza. The organism of trench fever is very definitely paired, each member of the pair consisting of a minute oval coccus-like body, and the surfaces opposed to each other are flattened—this produces a very typical picture under the microscope. The organism isolated in nephritis is rather larger, varying from 0·3 μ to 0·6 μ; it also is an oval coccus-like body, arranged usually in twos or in fours, and quite often in the form of short chains of four individuals. None of these four organisms show any differentiation on staining—no separation of an inner and an outer zone; but in some, as, for instance, in the organisms of trench fever and of nephritis, there is sometimes in cultures a suggestion of the presence of a capsule. This, however, is not confirmed either by staining or by dark-ground illumination. The organisms isolated in polyneuritis and in encephalitis are definite globoids varying in size from 0·2 μ to 0·5 μ. It may be useful to compare the size of these organisms with that of the better known bacteria.

It may be said that the average bacterium is approximately 2 μ long and 0·5 μ in diameter. One of the smallest organisms hitherto known is the reputed cause of influenza, the bacillus of Pfeiffer; this is said to be 1·5 μ long and 0·3 μ in diameter. It will be seen from this comparison that only the largest of the filter-passing organisms may reach as a maximum size the diameter or breadth of the average bacillus, and that some of the smallest—e.g. those of influenza and of rabies—are roughly one-tenth of the length of such a small organism as the bacillus of Pfeiffer. This comparison perhaps gives a better idea of the extreme smallness of these filter-passing organisms.

The question naturally arises as to whether these organisms exist in a still smaller form, or even in an ultra-microscopic form. In all cultures it is possible to see very small forms, so small that they cannot be measured, at any rate at present, and it is of course quite possible that the clearly visible forms are only better grown specimens of the smallest forms. On the other hand, the visible forms are very constant in their morphology in the different diseases, and can be recognized in the actual secretions and fluids of the body of the human patient and in the tissues of the experimental animals, as well as in the tissues of fatal human cases. Further, they can be recognized in the filtrates through suitable filters. Hence, although it is of course possible that ultra-microscopic forms may exist as a stage in the life-history of the particular organisms causing the diseases now under consideration, yet such a belief is not necessary for the explanation of the facts as they are at present known to us. The organism of influenza or of rabies can be seen in the filtrate obtained by filtering a culture of the virus through a porcelain Massen filter, hence it is not necessary to hypothecate an ultra-microscopic form in order to explain the fact that a living culture can be successfully obtained from such filtrates.

In rabies the organism has been obtained by direct culture from the brains of animals forwarded to us, in which rabies had been produced by inoculation in the laboratory. All the results therefore were obtained from laboratory animals proved by others to have died from rabies. This material was derived from both English and French sources, in the former from the recent outbreak in this country, in the latter the *virus fixé* of the Pasteur Institute. We have as yet had no opportunity of making direct observations on the disease as it occurs in nature. In the other five diseases the respective organisms have been isolated clinically by culture from the blood of patients during the febrile periods of the maladies, and it is of some interest that this blood culture should be successful not only in maladies like trench fever and influenza, where fever is a marked feature of the illness, but also in nephritis, encephalitis and polyneuritis, where fever is often only slight in amount and is liable to occur only in the initial stages of the disease. Although the febrile period of these diseases is that most suitable for the recovery of the organism from the blood, yet in several of these maladies it is possible to recover it during afebrile stages, and this is especially true of trench fever and of nephritis. The percentage of successful recoveries is, of course, much less than during the febrile period. In trench fever the virus can be recovered by culture in about 30 per cent of cases during the afebrile periods between the separate relapses. The blood, however, is not the only fluid from which these organisms can be recovered by culture. In influenza, for instance, it can be readily obtained from the sputum during the early stages of the disease, as it is then present in enormous numbers. In nephritis it can similarly be recovered from the urine, and in encephalitis from the cerebro-spinal fluid, but not in all cases. Up to the present time it has not been recovered from the cerebro-spinal fluid in cases of polyneuritis, but the number of cases investigated with this point in view has not been large. In influenza complicated with pleurisy or meningitis it can be obtained from the pleural or cerebro-spinal fluid, as the case may be.

These organisms can, however, not only be recovered by culture from the blood, secretions, or exudations of the body, but they can be actually seen in certain fluids and cells of the body. Thus, in influenza it can be seen, with suitable methods of staining, in the sputum during the early stages of the disease, and it is then present in large numbers; further, it can be seen in the free state in the pleural fluid of influenzal pleurisy, and also in the mononuclear leucocytes present in the pleural effusion. In nephritis the specific organism may be seen in considerable numbers in the centrifugalised deposit obtained from the urine. In trench fever the organism has been seen in stained films of the blood, in the plasma, in the red corpuscles, and in the mononuclear leucocytes. In encephalitis it has been seen in some cases in the cerebro-spinal fluid. These observations have some practical importance, inasmuch as they may assist in the diagnosis of some of the more obscure and difficult diseases, e.g. lethargic encephalitis.

Certain facts of interest are observed in regard to the distribution of these organisms in the tissues of fatal human cases. The globoid bodies have a wide distribution in the nervous system, inasmuch as they can be recovered from the brain, spinal cord, and such nerves as the sciatic. They are also found in the lymphatic glands, but not in the liver or spleen. In fatal cases of influenza the causative organism is very widely distributed in the lungs, heart, liver, spleen, kidney, brain and lymphatic glands. In rabies it is found in the nervous tissues, and also in the salivary and lymphatic glands. It would seem as if the lymphatic glands were a favourite site for these filter-passing organisms.

These organisms are difficult to stain satisfactorily, and the best results hitherto have been obtained by staining with 1 per cent methylene-blue, after washing the film with ether. It is also difficult to keep well-stained specimens for any length of time, as they are liable to fade. The organisms are Gram-positive in young cultures, but old cultures also contain many Gram-negative specimens.

All these six organisms are filter-passers in the usual sense attached to the term; they pass through certain filters, e.g. Berkefeld N & V, and through most Massen filters. Some of them, e.g. the polyneuritis organism, do not pass through the finest Massen filters; but the smallest, like those of rabies and influenza, pass through the finest Massen filters obtainable. In the case of the more porous Berkefeld filter, half-an-hour to an hour is required; with the Massen porcelain filter as much as three to four hours are necessary. It will suffice to say here that in all such experiments the filters are carefully cleaned, washed, sterilised and tested as to their permeability with fluids containing known organisms of known size. Finally, a negative pressure of something less than one atmosphere was used as an aspirating force.

Another point of general interest with regard to these viruses is their resistance, i.e. the facility or otherwise with which they are killed. It has long been known that the virus of certain diseases was extraordinarily resistant and remained potent for long periods. Thus the old physicians, who were shrewd observers, drew attention to the fact that the unknown poison of scarlet fever could remain dormant but potent for many years, and that scarlet fever had occurred as a result of wearing clothes that had formerly been worn by one who had died of scarlet fever, the garments then put away and the facts forgotten. In more recent times the virus of several diseases—e.g. horse sickness, rabies—has been proved in the laboratory to be very resistant to the action of antiseptics even when in contact with them, in some cases for months. Still more recently the American observers showed that the virus of poliomyelitis might remain potent for several years when kept in glycerine. The resistance of these viruses to the action of glycerine, which is very destructive to the more common bacteria, is a fact of the first importance, as it is partly owing to this property that it has been possible to grow these organisms from the blood and tissues. The resistance to glycerine varies in degree in different cases. Thus the viruses of polyneuritis, encephalitis and rabies are resistant for prolonged periods; in the case of polyneuritis and of encephalitis this may extend to many months. In the case of rabies it is not so prolonged, and with trench fever it is approximately three months. In influenza and nephritis the duration is much shorter, only a few weeks. The long duration of virulence in the case of the globoids is certainly a most remarkable fact, as cultures can be obtained from nerve tissue kept in 50 per cent glycerine for as long as twelve months, and the disease can also be reproduced in animals by suitable inoculation with such glycerinised material.

These viruses in culture are resistant to heat—e.g. to a temperature of 56° C. for thirty minutes; and their thermal death-point is between 65° C. and 70° C.

We will now consider shortly the pathogenic action of these filter-passing viruses. The well-defined group of globoids is characterised by producing lesions of the nervous system of a peculiar kind, lesions in which there is extensive destruction of the higher elements of the nervous system, such as the nerve-cells of the brain, spinal cord and ganglia. In addition, these organisms also produce lesions of the blood vessels of the nervous system. The extent of these different lesions varies in the different diseases, and at the present time there are at least three separate diseases of the nervous system—poliomyelitis, polyneuritis and encephalitis—due to the action of separate and distinct organisms of this group. It is not only interesting that all these nervous diseases are due to the pathogenic activities of globoids, but also that in the considerable number of filter-passing viruses isolated in the cause of the work at Etaples globoids have only been found as yet in these nervous diseases. Rabies, however, is a disease of the nervous system with not distant analogies to these other nervous diseases, but the organism isolated in rabies is not a globoid so far as we can say at present.

The other filter-passing viruses isolated and experimented on at Etaples produce well-marked and characteristic diseases, e.g. influenza, trench fever and nephritis. Trench fever is relatively to the others a mild disease, and we know nothing of the morbid anatomy and lesions of this disease, as such a thing as death from trench fever is unknown. In influenza the virus produces widespread lesions in the lungs and other organs of experimental animals, similar to those seen in man. Similarly in nephritis it is possible to reproduce the disease in the laboratory and so to study it in a way that was previously impossible. It will suffice to say here that all these viruses have to some degree a similar action; thus they all produce lesions of the smaller blood vessels, and many of them produce nephritis to some degree, but their action is also specific. Thus the virus of influenza produces its main effects on the lungs, heart and liver, and but little on the kidney; whereas the virus of nephritis produces but little effect on the lungs, none on the liver, and its main action is spent on the kidney. It was extremely fortunate that these researches were carried on concurrently, since in this way the results obtained with one virus served as a kind of control on those obtained with another virus grown under the same conditions but obtained from a different malady. In this way it was possible to obtain results rapidly as to the specific nature of the separate viruses.

The work done at Etaples dealt more especially with these six maladies—and in regard to them Koch's postulates were fulfilled—but in addition a series of observations was made on the possibility of detecting filter-passing organisms in other diseases, and more especially in the acute specific fevers. This group of diseases is one of the most important. Many of these diseases are those most dreaded, and are answerable for the heavy mortality of the young that we still suffer from. It was not possible, working as we did under the conditions obtaining with an army in the field, to do more than to begin this work, which it is hoped to carry farther in London in the immediate future. In the following diseases, amongst others, successful preliminary results have been obtained: mumps, rose measles, measles, scarlet fever, and typhus. In all of these filter-passing organisms have been successfully grown in culture and in subculture, but no experimental work proving that these organisms were the actual cause of the diseases in question has been possible as yet, but this it is hoped will soon be carried out.

In conclusion, some other facts of interest as to the mode of conveyance of some of the diseases dealt with here may be mentioned. The filter-passing virus of some of these diseases is unquestionably conveyed from the sick to the healthy through the agency of insects. This has been definitely proved to be the case in trench fever through the agency of the louse. Typhus is also conveyed by this insect.

Although the actual virus of yellow fever is unknown, unless Noguchi's recent work has solved the problem, yet there is distinct evidence that this disease is due to a filter-passer, and it is certainly conveyed by a mosquito.

In rabies the virus is conveyed directly by the bite of the rabid animal inoculating the victim with the saliva, and this has long been known to contain the virus, and now the virus can be grown by culture from the salivary glands.

In influenza the virus would seem to be conveyed directly from the sick to the healthy, and further, there is experimental evidence that the filter-passing virus can remain for long periods—e.g. months—in the tissues of an animal after experimental inoculation with the culture.

In poliomyelitis there is some dispute as to whether an insect agency plays any part, or whether the transmission is direct from the infected to the healthy.

In encephalitis, polyneuritis and nephritis the mode of conveyance of the virus is unknown.

[J. R. B.]

GENERAL MONTHLY MEETING,

Monday, June 2, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Captain Ralph A. Bagnold, R.E.
Mrs. Charles Cave,
Edmund Davis, J.P.
Miss Amelia Dorothy Defries,
William Edward Schall, B.Sc.
Frank Twyman,

were elected Members of the Royal Institution.

NOTICE is given that it is intended to take the necessary steps to restore the suspended Bye-Law, Chapter XVIII. Art. 2, and that the necessary Resolution will be moved at the General Monthly Meeting to be held on July 7, 1919, at 5 p.m.

WE, the UNDERSIGNED, propose that the Bye-Law, Chapter XVIII. Art, 2, which was suspended in 1916, be restored, and that the Friday Discourses in future be delivered at 9 o'clock, as they had been for a period of ninety years antecedent to 1916.

NOTE.—Bye-Law, Chapter XVIII. Art. 2, of the Weekly Meeting of the Members:—

> Art. 2. "These Meetings shall be held in the Library of Reference, which shall be open for that purpose on Friday Evenings, at Eight o'clock, and shut at Eleven."

(Signed)	James Crichton-Browne.	Leighton Jordan.
	Dundas Grant.	E. H. Hills.
	Horace T. Brown.	Hambleden.
	James Reid.	J. MacGregor Morris.
	Henry E. Armstrong.	Neville Bayly.
	H. R. Kempe.	E. E. Pearson.
	T. Wrightson.	J. W. Jarvis.
	Donald W. C. Hood.	J. Swinburne.
	James Dewar.	Maures Horner.

Royal Institution,
21 Albemarle Street, W.1.
2nd June, 1919.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Research Institute, Pusa: Bulletin, Nos. 80 & 83. 8vo. 1918–19.

Report on Progress of Agriculture in India for 1917–18. 8vo. 1919.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta, Rendiconti: Classe di Scienze Fisiche, Matematiche e Naturali, Vol. XXVIII. 1 Sem. Fasc. 3–8. 1919. 8vo.

American Geographical Society—Geographical Review, April 1919. 8vo.

Anglo-French Society—Villes Meurtries de France:—

Villes du Nord. Par L. Bocquet. 8vo. 1918.

Arras. Par H. Potez. 8vo. 1918.

The German School as a War Nursery. By V. H. Friedel. 8vo. 1918.

Alsace-Lorraine. By J. Duhem. 8vo. 1918.

Clemenceau. Par G. Geffroy. (French and English Text.) 8vo. 1919.

Europe's Liberation. By M. Clemenceau. (French and English Text.) 8vo. 1918.

France et Angleterre. Par E. Gosse. 8vo. 1917.

La Fête de l'Empire Brittanique. 8vo. 1918.

Astronomical Society, Royal—Monthly Notices, Vol. LXXIX. No. 5. 8vo. 1919.

Bankers, Institute of—Journal, Vol. XL. Part 5. 8vo. 1919.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXVI. No. 7. 4to. 1919.

British Astronomical Association—Journal, Vol. XXIX. No. 6. 8vo. 1919.

British Dental Association—Journal, Vol. XL. Nos. 9–10. 8vo. 1919.

Buenos Aires—Bulletin of Statistics, Jan.–Feb. 1919. 4to.

Cambridge Philosophical Society—Proceedings, Vol. XIX. Part 5. 8vo. 1919.

Canada, Department of Mines—Summary Report, 1917, Parts A & F. 8vo. 1918–19.

Memoir 96. 8vo. 1917.

Carnegie Institution of Washington—Annual Report of the Director of the Mount Wilson Solar Observatory, 1918. 8vo. 1919.

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Chemical Industry, Society of—Journal, May 1919. 8vo.

Chemical Society—Journal and Proceedings, May 1919. 8vo.

Chemistry, Institute of—Proceedings, 1919, Part 2. 8vo.

Colonial Institute, Royal—United Empire, Vol. X. No. 5. 8vo. 1919.

Department of Scientific and Industrial Research—Bulletins, Nos. 1–3. A Study on the Performance of Night Glasses. By L. C. Master. 8vo. 1919.

Report of Food Investigation Board, 1918. 8vo. 1919.

The Literature of Refrigeration. 8vo. 1919.

Editors—Aeronautical Journal, March 1919. 8vo.

Athenæum, May 1919. 4to.

Chemist and Druggist, May 1919. 8vo.

Church Gazette, May 1919. 8vo.

Dyer and Calico Printer, May 1919. 4to.

Electrical Times, May 1919. 4to.

Engineer, May 1919. fol.

Engineering, May 1919. fol.

General Electrical Review, April 1919. 8vo.

Horological Journal, May 1919. 8vo.

Illuminating Engineer, March–April 1919. 8vo.

Journal of Physical Chemistry, March–April 1919. 8vo.

Junior Mechanics, May 1919. 8vo.

Law Journal, May 1919. 8vo.

London University Gazette, May 1919. 4to.

Editors—continued.
Model Engineer, May 1919. 8vo.
Musical Times, May 1919. 8vo.
Nature, May 1919. 4to.
New Church Magazine, May–June 1919. 8vo.
Physical Review, April 1919. 8vo.
Science Abstracts, April 1919. 8vo.
Wireless World, June 1919. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LVII. No. 281. April 1919. 8vo.
Firenze, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani, March–April 1919. 8vo.
Firenze, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. XVI. Disp. 1. 8vo. 1919.
Franklin Institute—Journal, Vol. CLXXXVII. No. 5. 8vo. 1919.
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Quarterly Journal, Vol. LXXIV. Part 1. 8vo. 1919.
Gibson, G. W. E., Esq. (the Author)—Notes on "War-Time." By M. W. F. Treub. 8vo. 1919.
Janet, C., Esq. (the Author)—Sur la Phylogènese de l'Orthobionte. 8vo. 1916.
Lepper, G. H., Esq. (the Author)—From Nebula to Nebula, 4th edition. 8vo. 1919.
Linnean Society—Journal, Botany, Vol. XLIV. No. 298. 8vo. 1919.
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London Society—Journal, April 1919. 8vo.
Mechanical Engineers, Institution of—Proceedings, Oct.-Dec. 1918. 8vo. 1919.
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New York, Society for Experimental Biology—Proceedings, Vol. XVI. No. 5. 8vo. 1919.
Paris, Académie des Sciences—Comptes Rendus des Séances, Tome CLXIII. 4to. 1916.
Paris, Bureau International des Poids et Mesures—Travaux et Mémoires, Tome XVI. 4to. 1917.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, March–April 1919. 4to.
Pharmaceutical Society of Great Britain—Journal, May 1919. 8vo.
Photographic Society, Royal—Journal, Vol. LIX. No. 5. 8vo. 1919.
Physical Society of London—Proceedings, Vol. XXXI. Part 3. 8vo. 1919.
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Rome, Ministry of Public Works—Giornale del Genio Civile, Feb.-March 1919. 8vo.
Röntgen Society—Journal, Vol. XV. No. 59. 8vo. 1919.
Royal Engineers' Institute—Journal, Vol. XXIX. No. 6. 8vo. 1919.
Royal Society of Arts—Journal, May 1919. 8vo.
Royal Society of Canada—Transactions, Series 3, Vol. XII. 8vo. 1918–19.
Royal Society of London—Philosophical Transactions, A, Vol. CCXVIII. No. 565. 4to. 1919.
Proceedings, B, Vol. XC. No. 633. 8vo. 1919.
Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXV. No. 5. 8vo. 1919.
Smithsonian Institution—Report on U.S. National Museum, 1918. 8vo. 1919.
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Spielmann, Sir Isidore, C.M.G. (the Author)—Germany's Impending Doom—an Open Letter to Herr M. Harden. 8vo. 1918.
Statistical Society, Royal—Journal, Vol. LXXXII. Part 2. 8vo. 1919.
Swiss Chemical Society—Helvetica Chimica Acta, Vol. II. Fasc. 3. 8vo. 1919.
Tôhoku Imperial University, Sendai, Japan—Mathematical Journal, Vol. XV. Nos. 1-2. 8vo. 1919.
United Service Institution, Royal—Journal, Vol. LXIV. No. 454. 8vo. 1919.
United States Bureau of Standards—Bulletin, Vol. XIII. No. 4; Vol. XIV. Nos. 1-3. 8vo. 1917-18.
United States Department of Agriculture—Journal of Agricultural Research, Vol. XVI. Nos. 11-12; Vol. XVII. No. 1. 8vo. 1919.
Experiment Station Record, Vol. XL. Nos. 1-3. 8vo. 1919.
United States Patent Office—Official Gazette, Vol. CCLIX. No. 5; Vols. CCLX.-CCLXI. No. 2. 8vo. 1919.
Washington, National Academy of Sciences—Proceedings, Vol. V. No. 4. 8vo. 1919.
Zoological Society of London—Proceedings, 1918. 8vo.
Zurich Naturforschenden Gesellschaft — Vierteljahrsschrift, Band LXIII. Heft 3-4. 8vo. 1918.

WEEKLY EVENING MEETING,

Friday, June 6, 1919.

THE HON. SIR CHARLES A. PARSONS, K.C.B. J.P. M.A. LL.D. D.Sc. F.R.S., Manager and Vice-President, in the Chair.

PROFESSOR SIR ERNEST RUTHERFORD, LL.D. D.Sc. F.R.S. M.R.I.
(Nobel Laureate).

Atomic Projectiles and their Collisions with Light Atoms.

THE discovery of radio-activity has not only thrown a flood of light on the processes of transformation of radio-active atoms; it has at the same time provided us with the most powerful natural agencies for probing the inner structure of the atoms of all the elements. The swift α-particles and the high-speed electrons or β-rays ejected from radio-active bodies are by far the most concentrated sources of energy known to science. The enormous energy of the flying α-particle or helium atom is illustrated by the bright flash of light it produces when it impacts on a crystal of zinc sulphide, and by the dense distribution of ions along its trail through a gas. This great store of energy is due to the rapidity of its motion, which in the case of the α-particle from radium C (range 7 c.m. in air) amounts to 19,000 km. per second, or about 20,000 times the speed of a rifle-bullet. It is easily calculated that the energy of motion of an ounce of helium moving with the speed of the α-particle from radium C is equivalent to 10,000 tons of solid shot projected with a velocity of 1 km. per second.

In consequence of its great energy of motion the charged particle is able to penetrate deeply into the structure of all atoms before it is deflected or turned back, and from a study of the deflection of the path of the α-particle we are able to obtain important evidence on the strength and distribution of the electric fields near the centre or nucleus of the atom.

Since it is believed that the atom of matter is, in general, complex, consisting of positively and negatively charged parts, it is to be anticipated that a narrow pencil of α-particles, after passing through a thin plate of matter, should be scattered into a comparatively broad beam. Geiger and Marsden showed not only that much small scattering occurred, but also that in passing through the atoms of a heavy element some of the α-particles were actually turned back in their path. Considering the great energy of motion of the α-particle, this is an arresting fact, showing that the α-particle must encounter very intense forces in penetrating the structure of the atom. In order to explain such results, the idea of the nucleus atom was developed in which the main mass of the atom is concentrated in a positively charged nucleus of very small dimensions compared with the space occupied by the electrons which surround it. The scattering of α-particles through large angles was shown to be the result of a single collision where the α-particle passed close to this charged nucleus. From a study of the distribution of the particles scattered at different angles, results of first importance emerged. It was found that the results could be explained only if the electric forces between the α-particle and charged nucleus followed the law of inverse squares for distances apart of the order of 10^{-11} cm. Darwin pointed out that the variation of scattering with velocity was explicable only on the same law. This is an important step, for it affords an experimental proof that, at any rate to a first approximation, the ordinary law of force holds for electrified bodies at such exceedingly minute distances. It was also found that a resultant charge on the nucleus measured in fundamental units was about equal to the atomic number of the element. In the case of gold this number is believed from the work of Moseley to be 79.

Knowing the mass of the impinging α-particle and of the atom with which it collides, we can determine from direct mechanical principles the distribution of velocities after the collision, assuming that there is no loss of energy due to radiation or other causes. It is important to notice that in such a calculation we need make no assumption as to the nature of the atoms or of the forces involved in the approach and separation of the atoms. For example, if an α-particle collides with another helium atom, we should expect the α-particle to give its energy to the helium atom, which could thus travel on with the speed of the α-particle. If an α-particle collides directly with a heavy atom—e.g. of gold of atomic weight 197—the α-particle should retrace its path with only slightly diminished velocity, while the gold atom moves onward in the original direction of the α-particle, but with about one-fiftieth of its velocity. Next, consider the important case where the α-particle of mass 4 makes a direct collision with a hydrogen atom of mass 1. From the laws of impact, the hydrogen atom is shot forward with a velocity 1·6 times that of the impinging α-particle, while the α-particle moves forward in the same direction, but with only 0·6 of its initial speed. Marsden showed that swift hydrogen atoms set in motion by impact with α-particles can be detected like α-particles by the scintillations produced in a zinc sulphide crystal. Recently I have been able to measure the speed of such H atoms and found it to be in good accord with the calculated value, so that we may conclude that the ordinary laws of impact may be applied with confidence in such cases. The relative velocities of the α-particles and recoil atom after collision can thus be simply illustrated by impact of two perfectly elastic balls of masses proportional to the masses of the atoms.

While the velocities of the recoil atoms can be easily calculated, the distance which they travel before being brought to rest depends on both the mass and the charge carried by the recoil atom. Experiment shows that the range of H atoms, like the range of α-particles, varies nearly as the cube of their initial velocity. If the H atom carries a single charge, Darwin showed that its range should be about four times the range of the α-particle. This has been confirmed by experiment. Generally, it can be shown that the range of a charged atom carrying a single charge is mu^3R, where m is the atomic weight, and u the ratio of the velocity of the recoil atom to that of the α-particle, and R the range of the α-particle before collision. In comparison of theory with experiment, the results agree better if the index is taken as 2·9 instead of 3. If, however, the recoil atom carries a double charge after a collision, it is to be expected that its range would only be about one-quarter of the corresponding range if it carried a single charge. It follows that we cannot expect to detect the presence of any recoil atom carrying two charges beyond the range of the α-particle, but we can calculate that any recoil atom, of mass not greater than oxygen and carrying a single charge, should be detected beyond the range of the α-particle. For example, for a single charge the recoil atoms of hydrogen and helium should travel 4 R, lithium 2·8 R, carbon 1·6 R, nitrogen 1·3 R, and oxygen 1·1 R, where R is the range of the incident α-particles. We thus see that it should be possible to detect the presence of such singly charged atoms, if they exist, after completely stopping the α-particles by a suitable thickness of absorbing material. This is a great advantage, for the number of such swift recoil atoms is minute in comparison with the number of α-particles, and we could not hope to detect them in the presence of the much more numerous α-particles.

In order to calculate the number of recoil atoms scattered through any given angle from the direction of flight of the α-particles, it is necessary, in addition, to make assumptions as to the constitution of the atoms and as to the nature and magnitude of the forces involved in the collision. Consider, for example, the case of a collision of an α-particle with an atom of gold of nuclear charge 79. Assuming that the nucleus of the α-particle and that of the gold atom behave like point charges, repelling according to the inverse square law, it can readily be calculated that, for direct collision, the α-particle from radium C, which is turned through an angle of 180°, approaches within a distance $D = 3{\cdot}6 \times 10^{-12}$ cm. of the centre of the gold nucleus. This is the closest possible distance of approach of the α-particle, and the distance increases for oblique collisions. For example, when the α-particle is scattered through an angle of 150°, 90°, 30°, 10°, 5°, the closest distances of approach are 1·01, 1·2, 2·4, 6·2, 12 D respectively.

In the experiments of Geiger and Marsden, the number of α-particles scattered through 5° was observed to be about 200,000 times greater than the number through 150°. The variation with angle was in close accord with the theory, showing that the law of inverse squares holds for distances between $3 \cdot 6 \times 10^{-12}$ cm. and $4 \cdot 3 \times 10^{-11}$ cm. in the case of the gold atom. The experiments of Crowther in 1910 on the variation of scattering of β-rays with velocity indicate that a similar law holds also in that case, and for even greater distances from the nucleus.

We have seen that Marsden was able by the scintillation method to detect hydrogen atoms set in swift motion by α-particles up to distances about four times the range of the incident α-particle. In Marsden's experiments a thin-walled glass tube filled with radium emanation served as an intense source of rays. Since the lack of homogeneity of the α-radiation and the absorption in the glass are great drawbacks in making an accurate study of the laws controlling the production of swift atoms by impact, I have found it best to use for the purpose a homogeneous source of radium C by exposing a disc in a strong source of emanation. Fifteen minutes after removal from the emanation the α-rays from the disc are practically homogeneous, with a range in air of 7 cm. By special arrangements very intense sources of α-radiation can be produced in this way, and in the various experiments discs have been used the γ-ray activity of which has varied between 5 to 80 milligrams of radium. Allowance can easily be made for the decay of the radiation with time.

In the experiments with hydrogen the source was placed in a metal box about 3 cm. away from an opening in the end covered by a thin sheet of metal of sufficient thickness to absorb the α-rays completely. A zinc sulphide screen was mounted outside about 1 mm. away from the opening, so as to allow for the insertion of absorbing screens of aluminium or mica. The apparatus was filled with dry hydrogen at atmospheric pressure. The H atoms striking the zinc sulphide screen were counted by means of a microscope in the usual way. The strong luminosity due to the β-rays from radium C was largely reduced by placing the apparatus in a powerful magnetic field which bent them away from the screen.

If we suppose, for the distances involved in a collision, that the α-particle and hydrogen nucleus may be regarded as point charges, it is easy to see that oblique impacts should occur much oftener than head-on collisions, and consequently that the stream of H atoms set in motion by collisions should contain atoms the velocities of which vary from zero to the maximum produced in a direct collision. The slow-velocity atoms should greatly preponderate, and the number of scintillations observed should fall off rapidly when absorbing screens are placed in the path of the rays close to the zinc sulphide screen.

A surprising effect was, however, observed. Using α-rays of range 7 cm., the number of H atoms remained unchanged when the absorption in their path was increased from 9 cm. to 19 cm. of air equivalent. After 19 cm. the number fell off steadily, and no scintillations could be observed beyond 28 cm. air absorption. In fact, the stream of H atoms resembled closely a homogeneous beam of α-rays of range 28 cm., for it is well known that, owing to scattering, the number of α-particles from a homogeneous source begin to fall off some distance from the end of their range. The results showed that the H atoms are projected forward mainly in the direction of the α-particles and over a narrow range of velocity, and that few, if any, lower velocity atoms are present in the stream.

If we reduce the velocity of the α-particle by placing a metal screen over the source, it is found that the distribution of H atoms with velocity changes, and that the rays are no longer nearly homogeneous. When the range of the α-rays is reduced to 3·5 cm., the absorption of the H atoms is in close accord with the value to be expected from the theory of point charges. It is clear, therefore, that the distribution of velocity among the H atoms varies with the speed of the incident α-particles, and this indicates that a marked change takes place in the distribution and magnitude of the forces involved in the collision when the nuclei approach closer than a certain distance.

In addition to these peculiarities, the number of H atoms is greatly in excess of the number to be expected on the simple theory. For example, for the swiftest α-rays the number which is able to travel a distance equivalent to 10 cm. of air is more than thirty times greater than the calculated value. The variation in number of H atoms with velocity of the incident α-particle is also entirely different from that to be expected on the theory of point charges. The number diminishes rapidly with velocity, and is very small for α-particles of range 2·5 cm.

It must be borne in mind that the production of a high-speed H atom by an α-particle is an exceedingly rare occurrence. Under the conditions of the experiment the number of H atoms is seldom more than 1/30,000 of the number of α-particles. Probably each α-particle passes through the structure of 10,000 hydrogen molecules in traversing one centimetre of hydrogen at atmospheric pressure, and only one α-particle in 100,000 of these produces a high-speed H atom; so that in 10^9 collisions with the molecules of hydrogen the α-particle, on the average, approaches only once close enough to the centre of the nucleus to give rise to a swift hydrogen atom.

We should anticipate that for such collisions the α-particle is unable to distinguish between the hydrogen atom and the hydrogen molecule, and that H atoms should be liberated from matter containing free or combined hydrogen. This is fully borne out by experiment.

From the number of H atoms observed it can be easily calculated that the α-particle must be fired within a perpendicular distance of $2\cdot4 \times 10^{-13}$ cm. of the centre of the H nucleus in order to set it in swift motion. This is a distance less than the diameter of the electron, viz. $3\cdot6 \times 10^{-13}$ cm. The general results obtained with α-rays of range 7 cm. are similar to those to be expected if the α-particle behaves like a charged disc, of radius of about the diameter of an electron, travelling with its plane perpendicular to the direction of motion.

It is clear from the experiments with hydrogen that, for distances of the order of the diameter of the electron, the α-particle no longer behaves like a point charge, but that the α-particles must have dimensions of the order of that of the electron. The closest distance of approach in these collisions in hydrogen is about one-tenth the corresponding distances in the case of a collision of an α-particle with an atom of gold.

The results obtained with hydrogen in no way invalidate the nucleus theory as used to explain the scattering of α-rays by heavy atoms, but show, as we should expect, that the theory breaks down when we approach very close to the nucleus structure. In our ignorance of the constitution of the nucleus of the α-particle, we can only speculate as to its structure and the distribution of forces very close to it. If we take the α-particles of mass 4 to consist of four positively charged H nuclei and two negative electrons, we should expect it to have dimensions of the order of the diameter of the electron, supposing, as seems probable, that the H nucleus is of much smaller dimensions than the electron itself. When we consider the enormous magnitude of the forces between the α-particle and the H nucleus in a close collision—amounting to 6 kg. of weight—it is to be expected that the structure of the α-particle should be much deformed, and that the law of force may undergo very marked changes in direction and magnitude for small changes in the closeness of approach of the two colliding nuclei. Such considerations offer a reasonable explanation of the anomalies shown in the number and distribution with velocity of the H atoms exhibited for different velocities of the α-particles.

When we consider the enormous forces between the nuclei, it is not so much a matter of surprise that the nuclei should be deformed as that the structure of the α-particle or helium nucleus escapes disruption into its constituent parts. Such an effect has been carefully looked for, but so far no definite evidence of such a disintegration has been observed. If this be the case, the helium nucleus must be a very stable structure to stand the strain of the gigantic forces involved in a close collision.

We have seen that the recoil atoms of all elements of atomic mass less than 18 should travel beyond the range of the α-particle, provided they carry a single charge. Preliminary experiments, in which the α-particles passed through pure helium, showed that no long-range recoil atoms were present, indicating that after recoil the helium atom carries a double charge. In a similar way no certain evidence has been obtained of long-range recoil atoms from lithium, boron, or beryllium. It is difficult in experiments with solids or solid compounds to be sure of the absence of hydrogen or water-vapour, which results in the production of numerous swift H atoms. These difficulties are not present in the case of nitrogen and oxygen, and a special examination has been made of recoil atoms in these gases. Bright scintillations were observed in both these gases about 2 cm. beyond the range of the α-particle. These scintillations are, presumably, due to swift N and O atoms carrying a single charge, for the ranges observed are about those to be expected for such atoms. The scintillations due to recoil atoms of N and O are much brighter than H scintillations, although the actual energy of the flying atom is greater in the latter case. This difference in brightness is probably connected with the much weaker ionisation per unit of path due to the swifter H atom.

The corresponding range of the recoil atoms was about the same in oxygen, nitrogen, and carbon dioxide. Theoretically, it is to be anticipated that the N recoil atom should give a somewhat greater range than the O atom. The recoil atoms observed in carbon dioxide are apparently due to oxygen, for if the carbon atoms carried a single charge they should be detected beyond the range of O atoms.

The number of recoil atoms in nitrogen and oxygen and their absorption indicate that these atoms, like H atoms, are shot forward mainly in the direction of the α-particles. It is clear from the results that the nuclei of the atoms under consideration cannot be regarded as point charges for distances of the order of the diameter of the electron. Taking into account the close similarity of the effects produced in hydrogen and oxygen, and the greater repulsive forces between the nuclei in the latter case, it seems probable that the abnormal forces in the case of oxygen manifest themselves at about twice the distance observed in the case of hydrogen, i.e. for distances less than 7×10^{-13} cm. Such a conclusion is to be anticipated on general grounds, for presumably the oxygen nucleus is more complex and has larger dimensions than that of helium.

In his preliminary experiments Marsden observed that the active source always gives rise to a number of scintillations on a zinc sulphide screen far beyond the range of the α-particle. I have always found these natural scintillations present in the sources of radiation employed. The swift atoms producing these scintillations are deflected in a magnetic field, and have about the same range and energy as the swift H atoms produced by the passage of α-particles through hydrogen. The number of these natural scintillations is usually small, and it is very difficult to decide definitely whether such atoms arise from the disintegration of the active matter or are due to the action of the α-particles on hydrogen occluded in the source.

These natural scintillations were studied by placing the source in a closed box exhausted of air about 3 cm. from an opening in the end covered by a sheet of silver of sufficient thickness to stop the α-rays completely. The zinc sulphide screen was fixed outside close to the silver plate. On introducing dried oxygen or carbon dioxide into the vessel, the number of scintillations fell off in amount corresponding with the stopping power of the column of gas. An unexpected effect was, however, noticed on introducing dried air from the room. Instead of diminishing, the number of scintillations was increased, and for an absorption equivalent to 19 cm. of air the number was about twice that observed when the air was exhausted. It was clear from these results that the α-particles in their passage through air gave rise to long-range scintillations which appeared of about the same brightness as H scintillations. This effect in air was traced to the presence of nitrogen, for it was shown in dry, chemically prepared nitrogen as well as in air. The number of scintillations was much too large to be accounted for by the presence of traces of hydrogen or water-vapour, for the effect observed was equivalent to the number of H atoms produced by the mixture of hydrogen at 6 cm. pressure with oxygen. The measurements were always made well outside the range of the recoil nitrogen and oxygen atoms, which we have seen are stopped by 9 cm. of air.

These swift atoms which arise from nitrogen have about the same brightness and range as the H atoms produced from hydrogen, and, presumably, are charged hydrogen atoms Definite information on this point should be obtained by measuring the deflection of a pencil of these atoms in a magnetic and electric field. The experiments are, however, exceedingly difficult on account of the very small number of the scintillations to be expected under the experimental conditions. It should be mentioned that the evidence so far obtained is not sufficient to distinguish definitely whether these are H atoms or atoms of mass 2, 3, or 4, for the range and brightness of the latter would not be very different from those shown by the H atom.

It is difficult to avoid the conclusion that these long-range atoms arising from the collision of α-particles with nitrogen are not nitrogen atoms, but probably charged atoms of hydrogen or atoms of mass 2. If this be the case, we must conclude that the nitrogen atom is disintegrated under the intense forces developed in a close collision with swift α-particles, and that the atom liberated formed a constituent part of the nitrogen nucleus. It may be significant that from radio-active data we should expect the nitrogen nucleus of atomic mass 14 to consist of three helium nuclei of mass 4, and either two hydrogen nuclei or one nucleus of mass 2.

The effect produced in nitrogen would be accounted for if the H nuclei were outriders of the main nucleus of mass 12. The close approach of the α-particle leads to the disruption of its bond with the central nucleus, and under favourable conditions the H atom would acquire a high velocity and be shot forward like a free hydrogen atom. Taking into account the great energy of the particle, the close collision of an α-particle with a light atom seems to be the most likely agency to promote its disruption. Considering the enormous intensity of the forces brought into play in such collisions, it is not so much a matter of remark that the nitrogen atom should suffer disintegration as that the α-particle itself escapes disruption. The results, as a whole, suggest that if α-particles or similar projectiles of still greater energy were available for experiment, we might expect to break down the nucleus structure of many of the lighter atoms.

[E. R.]

GENERAL MONTHLY MEETING,

Monday, July 7, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Mrs. A. M. Henryson Caird,
Percy James Edmonds, M.B. B.Sc. M.R.C.S.

were elected Members.

The Special Thanks of Members were returned to Mr. A. B. Bence-Jones for his present of a Crayon Drawing by George Richmond of Dr. H. Bence-Jones, Secretary of the Royal Institution, 1860–73.

The Chairman announced the decease, on June 30, of The Right Hon. Lord Rayleigh, O.M., Honorary Professor of Natural Philosophy in the Royal Institution, and the following Resolution, passed by the Managers at their Meeting held this day, was unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution place on record in their Minutes their sense of the irreparable loss sustained by the Royal Institution and the community of Science by the death of the Right Honourable Lord Rayleigh, Honorary Professor of Natural Philosophy in the Royal Institution; Member of the Order of Merit; Member of His Majesty's Most Honourable Privy Council; Officer of the Legion of Honour; Chancellor of the University of Cambridge; Past-President of the Royal Society of London; Foreign Associate of the French Academy of Science; Doctor of Sciences and Laws of Numerous British and Foreign Universities; Corresponding Member of many Foreign Academies; Recipient of the Copley, Royal and Rumford Medals of the Royal Society; and many Medals awarded by other Societies; Nobel Laureate, etc.

Lord Rayleigh was a Member of the Royal Institution for more than half a century. He was appointed a Director of the Davy Faraday Research Laboratory of the Royal Institution from its foundation.

He succeeded Tyndall as Professor of Natural Philosophy in the Royal Institution, and held this Chair for eighteen years, during which time he delivered an Annual Course of Lectures along with a Friday Evening Discourse, and in the historic Laboratory of the Royal Institution he conducted experimental investigations and prepared his Lecture illustrations.

Lord Rayleigh on his retirement from the Professorship was elected Honorary Professor of Natural Philosophy, and frequently presided at the Friday Evening Meeting of the Members, originated by Faraday.

In 1896 he became Scientific Adviser to the Trinity House, thereby creating a Scientific continuity in this unique position—Faraday, Tyndall and Rayleigh.

While Professor at Cambridge he developed Practical Laboratory Training in Physics for Students, and the success of this method of teaching in the Cavendish Laboratory was largely conducive in founding the National Physical Laboratory, which has rendered the Empire such signal and important scientific services.

It is unnecessary to attempt to detail the vast and invaluable contributions Lord Rayleigh has made to Science for more than half a century. His Collected Scientific Papers have been published in five volumes by the University of Cambridge; and his Treatise on the Theory of Sound is acknowledged universally as the standard work on Acoustics.

On the occasion of the celebration of the Centenary of the Royal Institution he delivered a Commemorative Lecture before H.R.H. The Prince of Wales (King Edward VII.), in which he reviewed the great work of his predecessor, Thomas Young, the discoverer of the Undulatory Theory of Light.

During his Professorship he delivered nineteen Courses of Day Lectures, and the following twenty-three Friday Evening Discourses:—

The Dissipation of Energy	1875
The Explanation of Certain Acoustical Phenomena	1878
Water Jets and Water Drops	1885
Colours and Thin Plates	1887
Iridescent Crystals	1889
Foam	1890
Applications of Photography	1891
The Composition of Water	1892
Interference Bands and their Applications	1893
The Scientific Work of Tyndall	1894
Argon	1895
More about Argon	1896
The Limit of Audition	1897
Experiments with the Telephone	1898
Transparency and Opacity	1899
Flight	1900
Polish	1901
Interference of Sound	1902
Drops and Surface Tension	1903
Shadows	1904
The Law of Pressure of Gases below Atmosphere	1905
Colours of Sea and Sky	1910
Fluid Motions	1914

These Discourses, published in the Proceedings of the Royal Institution, show a variety of mathematical and experimental fertility in research, certainly for originality and profundity of learning unsurpassed by any other scientific investigator during the Victorian epoch.

On behalf of the Members of the Royal Institution the Managers desire to express their deepest sympathy with Lady Rayleigh and the family in their bereavement.

The Chairman announced the decease of Sir Boverton Redwood, Bart., on June 4, and of Sir John Brunner, Bart., on July 1, and the following Resolutions, passed by the Managers, were unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution place on record in their Minutes their sense of the great loss sustained by the Royal Institution in the death of Sir Boverton Redwood, Bart., D.Sc. F.R.S.E. F.C.E. F.I.C.

Technical Adviser on Petroleum and its Applications to the Admiralty, the Home Office, Government Departments, and many Public Bodies.

He was appointed Director of Petroleum Research, a branch of the Ministry of Munitions, during the War. His Treatise on Petroleum, which has reached its third edition, is universally recognised as a standard work on this subject.

Sir Boverton Redwood was a Member of the Board of Visitors, 1893–4, and a Member of the Royal Institution for thirty-five years. He delivered a Friday Discourse in 1918 on "The Romance of Petroleum," which contained an account of the vast progress made in the industrial use of petroleum since he began to develop this special branch of chemical technology into one of great national importance, and became one of the world's greatest authorities on the subject of mineral oils.

The Managers desire to express on behalf of the Members their deep sympathy with Lady Redwood and the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution place on record in their Minutes their sense of the loss the Institution has sustained by the death of the Right Honourable Sir John Tomlinson Brunner, Bart., P.C. D.L. LL.D.

Sir John Brunner was a Member for thirty years, and was keenly interested in the progress of Pure Science carried on in the Royal Institution and in the Davy Faraday Research Laboratory founded by his colleague the late Dr. Ludwig Mond.

In association with the late Dr. Ludwig Mond, Sir John Brunner was influential in introducing and developing the Ammonia Soda Process in Cheshire, which has become one of the most successful and largest alkali works in the world, and has rendered great service to the country in the preparation of chemical products for the conduct of the War.

Sir John Brunner fully recognised the necessity for the foundation of Local Universities in England by generously endowing three Professorial Chairs in the University of Liverpool, and advocated the importance of the cultivation of Pure Research as the basis for all real technical advancement.

The Managers desire to express on behalf of the Members their sympathy with Lady Brunner and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Agricultural Journal, Vol. XIV. No. 2. 8vo. 1919.
Bulletin of Agricultural Research Institute, Pusa, No. 86. 1919. 8vo.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta. Classe di Scienze Fisiche, Vol. XXVIII. 1° Sem. Fasc. 9. Classe di Scienze Morali, Vol. XXVII. Fasc. 7–10. 8vo. 1919.

Aeronautical Society, Royal—Journal, April 1919. 8vo.

Astronomer-Royal—Report to Board of Visitors of the Royal Observatory, 1919. 4to.

Astronomical Society, Royal—Monthly Notices, Vol. LXXIX. No. 6. 8vo. 1919.

Bankers, Institute of—Journal, Vol. XL. Part 6. 8vo. 1919.

British Architects, Royal Institute of—Journal, Vol. XXVI. No. 8. 8vo. 1919.

British Astronomical Association—Journal, Vol. XXIX. No. 7. 8vo. 1919.

British Dental Association—Journal, Vol. XL. Nos. 11–13. 8vo. 1919.

Chemical Industry, Society of—Journal, June 1919. 8vo.

Chemical Society—Journal and Proceedings, June 1919. 8vo.

Chevalier, M., Esq., and Soutter, J., Esq.—Various Books and Pamphlets on Bulgaria and Macedonia. 8vo. 1917–19.

Colonial Institute, Royal—United Empire, Vol. X. No. 6. 8vo. 1919.

Editors—Athenæum, June 1919. 4to.
Chemist and Druggist, June 1919. 8vo.
Church Gazette, June 1919. 8vo.
Dyer and Calico Printer, June 1919. 4to.
Electrical Review, June 1919. 4to.
Electrical Times, June 1919. 4to.
Engineer, June 1919. fol.
Engineering, June 1919. fol.
General Electric Review, May 1919. 8vo.
Horological Journal, June-July 1919. 8vc.
Journal of Physical Chemistry, May 1919. 8vo.
Junior Mechanics, June 1919. 8vo.
Law Journal, June 1919. 8vo.
London University Gazette, June 1919. 4to.
Model Engineer, June 1919. 8vo.
Musical Times, June 1919. 8vo.
Nature, June 1919. 4to.
Nuovo Cimento, Nov.-Dec. 1918. 8vo.
Science Abstracts, May 1919. 8vo.
Zoophilist, June 1919. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LVII. No. 282, May 1919. 8vo.
Engineers, Society of—Transactions, 1918. 8vo.
Franklin Institute—Journal, June 1919. 8vo.
Geographical Society, Royal—Journal, Vol. LIII. No. 6. 8vo. 1919.
Geological Society of London—Abstracts of Proceedings, Nos. 1041-1042. 8vo. 1919.
George Williams Hooper Foundation for Medical Research—Collected Reprints, Vol. III. 1917-18. 8vo. 1919.
Horticultural Society, Royal—Journal, Vol. XLIV. May 1919. 8vo.
Indian Association for the Cultivation of Science—Proceedings, Vol. IV. Part 3. 8vo. 1919.
London County Council—Gazette, June 1919. 4to.
Marconi's Wireless Telegraph Co.—Year-Book of Wireless Telegraphy, 1919. 8vo.
Meteorological Office—Daily Readings, April 1919. 4to.
Microscopical Society, Royal—Journal, 1919, Part 1. 8vo. 1919.
Monaco, l'Institut Océanographique—Bulletin, Nos. 352-353, 355. 8vo. 1919.
New York, Society for Experimental Biology—Proceedings, Vol. XVI. No. 6. 8vo. 1919.
New Zealand—Statistics, 1917, Vol. III. 4to. 1918.
Paris, Société Française de Physique—Journal de Physique, July-Oct. 1917. 8vo.
Pharmaceutical Society of Great Britain—Journal, June 1919. 8vo.
Photographic Society, Royal—Journal, Vol. LIX. No. 6. 8vo. 1919.
Physical Society—Proceedings, Vol. XXXI. Part 4. 8vo. 1919.
Quekett Microscopical Club—Journal, Series 2, Vol. XIV. No. 84, April 1919. 8vo.
Rio de Janeiro, Observatorio Nacional—Annuario, 1919. 8vo. 1918.
Royal Engineers' Institute—Journal, Vol. XXX. No. 1. 8vo. 1919.
Royal Society of Arts—Journal, June 1919. 8vo.
Royal Society of Edinburgh—Transactions, Vol. LII. Part 2. 4to. 1919.
Proceedings, Vol. XXXVIII. Part 3; Vol. XXXIX. Part 1. 8vo. 1918-19.
Royal Society of London—Proceedings, A, Vol. XCV. No. 672; B, Vol. XC. No. 634. 8vo. 1919.
Philosophical Transactions: A, Vol. CCXVIII. Nos. 566-568. 4to. 1919.
Royal Society of New South Wales—Proceedings, Vol. LI. 8vo. 1917.

Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXV. Nos. 6–7. 8vo. 1919.

Smithsonian Institution—Miscellaneous Collections, Vol. LXIX. No. 9. 8vo. 1919.

Società degli Spettroscopisti Italiani—Memorie, Vol. VIII. Serie 2, Disp. 1–3. 4to. 1919.

Statistical Society, Royal—Journal, Vol. LXXXII. Part 3. 8vo. 1919.

Sweden, Royal Academy of Sciences—S. Klingenstiernas Levnad och verk. By H. Hildebrandsson. Part 1. 8vo. 1919.

Swiss Chemical Society—Helvetica Chimica Acta, Vol. II. Fasc. 4. 8vo. 1919.

Tôhoku Imperial University, Japan—Mathematical Journal, Vol. XV. Nos. 3–4. 8vo. 1919.

Science Reports, Vol. VIII. No. 1. 8vo. 1919.

United States Department of Agriculture—Experiment Station Record, Vol. XL. Nos. 4–5. 8vo. 1919.

United States Patent Office—Official Gazette, Vol. CCLXI. No. 3.—Vol. CCLXII. 8vo. 1919.

GENERAL MONTHLY MEETING,

Monday, November 3, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

Ralph Griffin,
Lieut.-Col. A. E. Le Rossignol,
The Right Hon. Lord Rayleigh, Sc.D. F.R.S.
Edmund Basil Wedmore, M.Inst.E.E.

were elected Members of the Royal Institution.

The following Resolution, passed at the General Meeting on July 2, 1919, was duly confirmed :—

> RESOLVED, That the Bye Law Chap. XVIII. Art. 2, which was suspended in 1916, be restored, and that the Friday Discourses in future be delivered at 9 o'clock, as they had been for a period of ninety years antecedent to 1916.

The Special Thanks of the Members were returned to :—

Richard Pearce, Esq., M.R.I., for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures ;

Robert Mond, Esq., J.P. M.A. M.R.I., for his gift of Laboratory Material ;

Sir Humphry Davy Rolleston, K.C.B., for his gift of a Drawing of Sir Humphry Davy's birthplace, and a Watercolour of his statue in Market Jew Street, Penzance.

The Chairman announced the decease, on July 2, 1919, of Professor A. P. N. Franchimont, an Honorary Member, and the following Resolution, passed by the Managers at their Meeting held this day, was unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution through the decease of Professor Antoine Paul Nicholas Franchimont, Ph.D., Officer of the Legion of Honour, Honorary Fellow of the Chemical Society, Emeritus Professor of Organic Chemistry in the University of Leyden, and an Honorary Member of the Royal Institution. Professor Franchimont was appointed Professor of Chemistry in the University of Leyden in 1874, and during his forty years' tenure of office was the most brilliant Dutch investigator in the field of Organic Chemistry. He was the author of more than sixty papers on the properties and characteristics of the Nitro-Amides and the Aliphatic Nitramines, and other allied subjects. Technical Science is indebted to him for the application of sulphuric acid and zinc chloride as catalysts in acetylation. He was one of the founders, and the editor since 1883, of the Dutch National Chemical Journal called *Recueil des Travaux Chimiques des Pays-Bas.*

The Managers desire to express, on behalf of the Members, their sincere sympathy with Madame Franchimont and the family in their bereavement.

The **Presents** received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Records of the Geological Survey, Vol. XLIX. Part 4; Vol. L. Parts 1-2. 8vo. 1919.
Agricultural Journal of India, Vol. XIV. Parts 3-4. 8vo. 1919.
British Museum (Natural History)—Guide to British Fresh-Water Fishes. 8vo. 1919.
Economic Pamphlets, Nos. 3-7. 8vo. 1916-18.
British Lichens. By L. Smith. 8vo. 1919.
Rats and Mice as Enemies of Mankind. 8vo. 1919.
Distribution in England and Wales of Anophiline Mosquitoes. 8vo. 1919.
Aeronautical Society, Royal—Aeronautical Journal, May-Sept. 1919. 8vo.
Glossary of Aeronautical Terms. 8vo. 1919.
Agricultural Society, Royal—Journal, Vol. LXXIX. 8vo. 1918.
American Geographical Society—Geographical Review, May-July 1919. 8vo.
American Philosophical Society—Proceedings, Vol. LVII. No. 7. 8vo. 1918.
Antiquaries, Society of—Proceedings, Second Series, Vol. XXX. 8vo. 1919.
Asiatic Society, Royal—Journal, April-July, 1919. 8vo.
Astronomer Royal—Report of H.M. Astronomer, Cape of Good Hope, 1918. 4to. 1919.
Astronomical Society, Royal—Monthly Notices, Vol. LXXIX. No. 7. 8vo. 1919.
List of Fellows, 1919. 8vo.
Australia, Agent-General—Report of Department of Mines, 1916-17. 8vo. 1919.
Australia, Commonwealth Institute of Science and Industry—Science and Industry, Vol. I. Nos. 1-4. 8vo. 1919.
Bulletin, Nos. 12-13. 8vo. 1919.
Bankers, Institute of—Journal, Vol. XL. Part 7. 8vo. 1919.
Basle, Naturforschenden Gesellschaft—Verhandlungen, Band XXIX. 8vo. 1918.
Boston Public Library—Sixty-Seventh Annual Report, 1918-19. 8vo. 1919.
Bulletin, Fourth Series, Vol. I. No. 2. 8vo. 1919.
Botanic Society, Royal—Quarterly Summary, No. 1, July. 1919. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXVI. Nos. 9-12. 4to. 1919.
British Association for the Advancement of Science—Report for 1918. 8vo. 1919.
British Astronomical Association — Journal, Vol. XXIX. Nos. 8-9. 8vo. 1919.
British Dental Association—Journal, Vol. XL. Nos. 14-20. 1919. 8vo.
Buenos Aires—Bulletin of Statistics, March-April 1919. 4to.
Cambridge Observatory—Observations, Vol. XXV. 4to. 1919.
Canada, Department of Mines—Bulletin, No. 28. 8vo. 1919.
Annual Report on Mineral Production, 1917. 8vo. 1919.
Memoirs, Nos. 107-111. 8vo. 1919.

Canadian Government—Debates of the Senate, 1919, No. 10. 8vo.
Canada Year-Book, 1918. 8vo. 1919.
Canadian Institute, Royal—Transactions, Vol. XII. Part 1. 8vo. 1919.
Carnegie Endowment for International Peace—Second Pan-American Scientific Congress. 8vo. 1916.
Conference of American Teachers of International Law. 8vo. 1914.
Carnegie Institution—Contributions from the Mount Wilson Solar Observatory, Nos. 160-166. 8vo. 1919.
Chemical Industry, Society of—Journal, July-Oct. 1919. 8vo.
Chemical Society—Journal and Proceedings for July-Oct. 1919. 8vo.
List of Fellows, 1919. 8vo.
Chicago, John Crerar Library—Twenty-Fourth Annual Report. 8vo. 1919.
Civil Engineers, Institution of—Abstracts of Papers in Scientific Transactions and Periodicals, New Series, No. 1. 8vo. 1919.
Clarke, Messrs. J. W.—Hampton's Scholastic Dictionary, 1918-19. 8vo.
Colocotronis, V. (the Author)—La Macédoine et l'Hellenisme. 8vo. 1919.
Colonial Institute, Royal—United Empire, Vol. X. Nos. 7-10. 8vo. 1919.
Dewar, Sir James, LL.D. F.R.S. M.R.I.—The Question. By E. Clodd. 8vo. 1917.
Memorial Celebration: Carnegie Institution. 4to. 1907.
Anorganischen Chemie von H. Erdmann, 4th edition. 8vo. 1906.
Aluminium. By J. W. Richards. 1st edition. 8vo. 1887.
East India Association—Journal, N.S., Vol. X. No. 3. 8vo. 1919.
Editors—Animals' Defender, July-Oct. 1919. 8vo.
Athenæum, July-Oct. 1919. 4to.
Author, July-Oct. 8vo. 1919.
Chemist and Druggist, July-Oct. 1919. 8vo.
Church Gazette, July-Oct. 1919. 8vo.
Collegian, Nos. 1-5. 8vo. 1919.
Dyer and Calico Printer, July-Oct. 1919. 4to.
Engineer, July-Oct. 1919. fol.
Engineering, July-Oct. 1919. fol.
General Electric Review, June-Aug. 1919. 8vo.
Horological Journal, July-Oct. 1919. 8vo.
Illuminating Engineer, May-Aug. 1919. 8vo.
Journal of Physical Chemistry, June-Oct. 1919. 8vo.
Junior Mechanics, July-Oct. 1919. 8vo.
Law Journal, July-Oct. 1919. 8vo.
London University Gazette, July-Oct. 1919. 4to.
Model Engineer, July-Oct. 1919. 8vo.
Musical Times, July-Oct. 1919. 8vo.
Nature, July-Oct. 1919. 4to.
New Church Magazine, July-Oct. 1919. 8vo.
Nuovo Cimento, Jan.-Sept. 1919. 8vo.
Physical Review, May-Aug. 1919. 8vo.
Science Abstracts, June-Sept. 1919. 8vo.
Terrestrial Magnetism, Vol. XXIV. No. 2. 8vo. 1919.
The Quest, Oct. 1919. 8vo.
Wireless World, July-Oct. 1919. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LVII. Nos. 283-285. 1919. 8vo.
Engineers, Society of—List of Members, 1919. 8vo.
Essex Archæological Association—Transactions, Vol. XV. Part 2. 8vo. 1919.
Faraday Society—Proceedings, Vol. XIV. Part 3. 8vo. 1919.
Fleming, J. A., D.Sc. F.R.S. M.R.I. (the Author)—The Thermionic Valve. 8vo. 1919.
Florence, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani, May-Aug. 1919. 8vo.

Formosa, Bureau of Productive Industries—Icones Plantarum Formosanarum. By B. Hayata. Vol. VIII. 8vo. 1919.

Franklin Institute—Journal, July–Sept. 1919. 8vo.

Geographical Society, Royal—Journal, July–Oct. 1919. 8vo.

Geological Society of London—Quarterly Journal, Vol. LXXIV. Parts 2–3. 8vo. 1919.

Geological Survey of Great Britain—Summary of Progress, 1918. 8vo. 1919.

Greek Bureau of Foreign Information—Vindication of Greek National Policy, 1912–17. 8vo. 1919.

Redemption of St. Sophia. By J. A. Douglas. 8vo. 1919.

Harvard College—Astronomical Observatory, Seventy-Third Annual Report, 1918. 8vo. 1919.

Imperial Arts League—Journal, June 1919. 8vo.

Imperial Institute—Bulletin, Vol. XVII. No. 1. 8vo. 1919.

Indian Association for the Cultivation of Science—Proceedings, Vol. IV. Part 4. 8vo. 1918.

Iron and Steel Institute—Journal, Vol. XCIX. 8vo. 1919.

List of Members and Associates. 8vo. 1919.

Jordan, H. W. (the Author)—Private Companies. 8vo. 1919.

Reminders for Company Secretaries. 8vo. 1919.

Keith, A., M.D. F.R.S. M.R.I. (the Author)—Menders of the Maimed. 8vo. 1919.

Keith, J., M.R.I. (the Author)—Modern Fans, Blowers and Boosters. 8vo. 1919.

Kyoto Imperial University—Memoirs of the College of Science, Vol. III. Nos. 5–10. 8vo. 1919.

Life-Boat Institution, Royal National—The Life-Boat, Sept. 1919. 8vo.

Annual Report, 1918. 8vo. 1919.

Linnean Society—Journal, Zoology, Vol. XXXIII. No. 224; Botany, Vol. XLIV. No. 299. 8vo. 1919.

Lisbon, Royal Academy of Sciences—Boletim da Segunda Classe, Vol. XI. Fasc. 1–2. 8vo. 1917–18.

História e Memórias, Tome XIII. 4to. 1913.

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GENERAL MONTHLY MEETING,

Monday, December 1, 1919.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

William Wallace Campbell, Sc.D. LL.D. (California),
Jacques Loeb, M.D. D.Sc. (New York),
Robert Andrews Millikan, M.A. Ph.D. Sc.D. (Chicago),
Pierre Paul Emile Roux, M.D., Membre de l'Institut, Directeur l'Institut Pasteur (Paris),
Paul Sabatier, Membre de l'Academie des Sciences, Officier de la Légion d'Honneur (Toulouse),
Arthur Gordon Webster, D.Sc. LL.D. (Harvard),

were elected Honorary Members of the Royal Institution.

John Holgate Batten,
James William Butler,
Philip Ray Coursey,
Mrs. Caroline M. Dunbar,
Arthur Holland,
Charles Coxhead Lindsay,
Mrs. Katherine A. Riches,
Arthur Ruck,
Lieut.-Col. Arthur Cecil Williams, C.B.E. R.A.

were elected Members.

The following Lecture Arrangements Before Easter 1920 were announced:—

PROFESSOR W. H. BRAGG, C.B.E. D.Sc. F.R.S., Quain Professor of Physics, University of London. Six Lectures, adapted to a Juvenile Auditory, on THE WORLD OF SOUND: 1. WHAT IS SOUND?; 2. SOUND AND MUSIC; 3. SOUNDS OF THE COUNTRY; 4. SOUNDS OF THE TOWN; 5. SOUNDS OF THE SEA; 6. SOUNDS IN WAR. On Dec. 30, 1919 (*Tuesday*); Jan. 1, 3, 6, 8, 10, 1920.

SIR JOHN CADMAN, K.C.M.G. D.Sc. M.Inst.C.E., Professor of Mining, University, Birmingham. Two Lectures on 1. MODERN DEVELOPMENT OF THE MINER'S SAFETY LAMP; 2. PETROLEUM AND THE WAR. On *Tuesdays*, Jan. 13, 20.

PROFESSOR G. ELLIOT SMITH, M.D. F.R.S. F.R.C.P., Professor of Anatomy, University of London. Three Lectures on THE EVOLUTION OF MAN AND THE EARLY HISTORY OF CIVILIZATION: 1. MAN'S ORIGIN; 2. ELEPHANTS AND ETHNOLOGISTS; 3. THE SEARCH FOR GOLD. On *Tuesdays*, Jan. 27, Feb. 3, 10.

PROFESSOR ERNEST WILSON, M.Inst.C.E. M.I.E.E., Dean of the Faculty of Engineering, King's College, London. Two Lectures on MAGNETIC SUSCEPTIBILITY. On *Tuesdays*, Feb. 17, 24.

PROFESSOR ARTHUR KEITH, M.D. LL.D. F.R.S. F.R.C.S. M.R.I., Fullerian Professor of Physiology, Royal Institution. Four Lectures on BRITISH ETHNOLOGY: THE INVADERS OF BRITAIN. On *Tuesdays*, March 2, 9, 16, 23.

RICHARD R. TERRY, Mus.Doc. F.R.C.O., Organist and Director of the Music, Westminster Cathedral. Three Lectures (with Musical Illustrations) on RENAISSANCE MUSIC IN ITALY AND ENGLAND. On *Thursdays*, Jan. 15, 22, 29.

PROFESSOR A. E. CONRADY, Professor of Optical Design, Imperial College of Science, London. Two Lectures on RECENT PROGRESS IN APPLIED OPTICS. On *Thursdays*, Feb. 5, 12.

ARTHUR H. SMITH, M.A. F.S.A., Keeper of Greek and Roman Antiquities, British Museum. Two Lectures on ILLUSTRATIONS OF ANCIENT GREEK AND ROMAN LIFE IN THE BRITISH MUSEUM. On *Thursdays*, Feb. 19, 26.

LIEUT.-COL. ERNEST GOLD, R.E. D.S.O. F.R.S. Two Lectures on THE UPPER AIR: 1. MODERN METHODS OF INVESTIGATION, AND THEIR APPLICATION IN THE WAR; 2. RESULTS AND THEIR INTERPRETATION. On *Thursdays*, March 4, 11.

STEPHEN GRAHAM, Author of "A Private in the Guards." Two Lectures on 1. THE SPIRIT OF AMERICA AFTER THE WAR; 2. THE HOPE FOR RUSSIA. On *Thursdays*, March 18, 25.

ALFRED NOYES. Two Lectures on 1. THE ANGLO-AMERICAN BOND OF LITERATURE; 2. ASPECTS OF MODERN POETRY. On *Saturdays*, Jan. 17, 24.

SIR FRANK WATSON DYSON, LL.D. F.R.S., The Astronomer Royal. Three Lectures on THE ASTRONOMICAL EVIDENCE BEARING ON EINSTEIN'S THEORY OF GRAVITATION: 1. MOVEMENT OF THE PERIHELION OF MERCURY; 2. DISPLACEMENT OF SOLAR SPECTRAL LINES; 3. DEFLECTION OF LIGHT IN THE SUN'S GRAVITATIONAL FIELD. On *Saturdays*, Jan. 31, Feb. 7, 14.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. D.Sc. Pres.R.S. M.R.I., The Master of Trinity, Professor of Natural Philosophy, Royal Institution. Six Lectures on POSITIVE RAYS. On *Saturdays*, Feb. 21, 28, March 6, 13, 20, 27.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Bulletin of Agricultural Research Institute, Pusa, No. 90. 1919. 8vo.
Memoirs of the Department of Agriculture: Botanical Series, Vol. X. Nos. 2-3; Chemical Series, Vol. V. No. 5. 8vo. 1919.
Report on Madras Government Museum, 1918-19. 4to. 1919.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta. Classe di Scienze Fisiche, Vol. XXVIII. 1° Sem. Fasc. 11-12, 2° Sem. Fasc. 1-2. Classe di Scienze Morali, Vol XXVII. Fasc. 11-12. 8vo. 1919.

Aeronautical Society, Royal—Journal, October 1919. 8vo.

American Geographical Society—Geographical Review, Aug.-Sept. 1919. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. XXIX. No. 9. 8vo. 1919.

Athenæum Club—List of Members, 1918-19. 8vo.

Australia, Commonwealth of—Science and Industry, Sept. 1919. 8vo.

Institute of Science Bulletin, No. 14. 8vo. 1919.

Bankers, Institute of—Journal, Vol. XL. Part 8. 8vo. 1919.

Boston Public Library—Bulletin, Fourth Series, Vol. I. No. 3. 8vo. 1919.

Botanic Society, Royal—Quarterly Summary, No. 2, Oct. 8vo. 1919.

British Architects, Royal Institute of—Journal, Vol. XXVII. Nos. 1-2. 8vo. 1919.

British Astronomical Association—Journal, Vol. XXX. No. 1. 8vo. 1919.

British Dental Association—Journal, Vol. XL. Nos. 21-22. 8vo. 1919.

Buchanan, J. Y., F.R.S. M.R.I. (the Author)—Accounts Rendered. 8vo. 1919.

Canada, Department of Mines—Geological Survey: Summary Report, 1918, Parts C-G. 8vo. 1919.

Memoirs, Nos. 111 and 114. 8vo. 1919.

Chemical Industry, Society of—Journal, Nov. 1919. 8vo.

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Colonial Institute, Royal—United Empire, Vol. X. No. 11. 8vo. 1919.

Columbia University—Researches in Physical Optics. By R. W. Wood. Part 2 4to. 1919.

East India Association—Journal, Vol. X. No. 4. 8vo. 1919.

Editors—Animals' Defender, Nov. 1919. 4to.

Athenæum, Nov. 1919. 4to.

Chemist and Druggist, Nov. 1919. 8vo.

Church Gazette, Nov. 1919. 8vo.

Dyer and Calico Printer, Nov. 1919. 4to.

Engineer, Nov. 1919. fol.

Engineering, Nov. 1919. fol.

General Electric Review, Sept. 1919. 8vo.

Horological Journal, Nov. 1919. 8vo.

Illuminating Engineer, Sept. 1919. 8vo.

Junior Mechanics, Nov. 1919. 8vo.

Law Journal, Nov. 1919. 8vo.

London University Gazette, Nov. 1919. 4to.

Model Engineer, Nov. 1919. 8vo.

Musical Times, Nov. 1919. 8vo.

Nature, Nov. 1919. 4to.

New Church Magazine, Nov.-Dec. 1919. 8vo.

Physical Review, Sept.-Oct. 1919. 8vo.

Science Abstracts, Oct 1919. 8vo.

Wireless World, Nov. 1919. 8vo.

Filow, B. D. (the Author)—Early Bulgarian Art. 4to. 1919.

Florence, Biblioteca Nazionale Centrale di Firenze—Bollettina, Oct.-Nov. 1919. 8vo.

Franklin Institute—Journal, Oct.-Nov. 1919. 8vo.

1919 Year Book. 8vo. 1919.

Geographical Society, Royal—Journal, Vol. LIV. No. 5. 8vo. 1919.

Geological Society of London—Abstracts of Proceedings, Nos. 1043-1044. 8vo. 1919.

Johns Hopkins University—American Journal of Philology, Vol. XL. No. 3. 8vo. 1919.

Linnean Society Proceedings 131st Session. 8vo. 1919.

List of Fellows, 1919. 8vo.

London County Council—Gazette, Nov. 1919. 4to.

London Society—Journal, Oct. 1919. 8vo.

Manchester Literary and Philosophical Society—List of Members, 1919. 8vo.

Mexico, Government of—Autografos de Morelos. fol. 1918.

New York State College of Agriculture — Annual Reports of Experiment Station, 1914–17, 4 vols. 8vo. 1915–18.

Paris, Société Française de Physique—Journal de Physique, Feb. 1919. 8vo.

Pharmaceutical Society of Great Britain—Journal, Nov. 1919. 8vo.

Photographic Society, Royal—Journal; Vol. LIX. No. 7. 8vo. 1919.

Royal Engineers' Institute—Journal, Vol. XXX. No. 6. 8vo. 1919.

Royal Society of Arts—Journal, Nov. 1919. 8vo.

Royal Society of London—Philosophical Transactions: B, Vol. CCIX. Nos. 363–365. 4to. 1919.

Scottish Geographical Society, Royal—Scottish Geographical Magazine, Vol. XXXV. No. 9. 8vo. 1919.

Tôhoku Imperial University, Japan—Science Reports, Second Series, Vol. V. No. 2. 4to. 1919.

United States Department of Agriculture — Experiment Station Record, Vol. XLI. Nos. 1–3. 8vo. 1919.

Journal of Agricultural Research, Vol. XVIII. Nos. 1–2. 8vo. 1919.

United States Patent Office—Official Gazette, Vol. CCLXVII.–Vol. CCLXVIII. No. 2. 8vo. 1919.

Western Australia—Quarterly Statistical Abstract, No. 213, March 1919. 8vo.

Yorkshire Philosophical Society—Annual Report, 1918. 8vo. 1919.

Zoological Society of London—Proceedings, 1919, Parts 1–2. 8vo. 1919.

WEEKLY EVENING MEETING,

Friday, March 21, 1919.

THE HON. RICHARD CLERE PARSONS, M.Inst.C.E., Vice-President, in the Chair.

PROFESSOR W. W. WATTS, LL.D. D.Sc. F.R.S.

Fossil Landscapes.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, April 11, 1919.

LORD ROTHSCHILD, F.R.S., in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. Pres. R.S. M.R.I., The Master of Trinity.

Piezo Electricity and its Applications.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, January 17, 1919.

COLONEL E. H. GROVE-HILLS, C.M.G. C.B.E. D.Sc. F.R.S.,
Secretary and Vice-President, in the Chair.

SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. M.R.I.,
Fullerian Professor of Chemistry.

Liquid Oxygen in Warfare.

BY a resolution of the Managers of the Royal Institution on July 5, 1915, it was decided that the Professoriate and Staff should be at the disposal of the Government, so far as they could usefully direct their attention to help in the prosecution of the war. Some of the work described is a result of this Resolution: all costs being borne by the Royal Institution.

When comparing the general aspect of the growth of chemical industry in England and in Germany in the Presidential address delivered to the British Association at Belfast in 1902, it was pointed out that, notwithstanding the immense range of industries in the United Kingdom and the abundance of raw material at our disposal, we could not, at that time, show more than one-third of the professional staff employed in Germany. The appalling fact was not that Germany had seized one or two Chemical Industries, but that the Germans had reached a point of general training and specialised equipment that would take us two generations of hard educational work to attain.*

PRACTICAL EFFECTS OF LOW TEMPERATURE RESEARCH.

The liquefaction of oxygen by previous compression at the temperature of liquid ethylene in the Laboratories of the Royal Institution in 1884 † was followed by the introduction of commercial methods of producing liquid atmospheric air from which oxygen was obtained by fractional distillation. Of the total production of about 120 tons of liquid air per day in this country, 53 per cent. was used in the oxy-acetylene blowpipe for cutting armour plates and for welding. Some photographs were exhibited illustrating how these operations were carried out by the British Oxygen Company.

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* NOTE, 1922.—The Lecturer has lived to see one generation pass and we are now only beginning our labours.—J. D.

† Proc. Roy. Inst., xi. 148.

UTILISATION OF AIR NITROGEN.

For a long time oxygen alone was in industrial demand, and there was no use for the huge quantities of nitrogen obtained simultaneously.

Processes for the fixation of nitrogen and the production of fertilisers and nitric acid were soon developed. The German dye makers exploited electrical methods of fixation, and spent some three millions sterling in acquiring water power in Sweden for the production of the necessary cheap energy.

Then came Haber's discovery that when a mixture of 3 volumes hydrogen and 1 of nitrogen under 200 to 300 atmospheres pressure, at a temperature rather below red heat, is passed over a suitable catalytic agent (osmium was used first, but was ultimately replaced by finely divided iron), the issuing stream of gas contains a proportion of ammonia, dependent on the temperature and pressure, which can be absorbed out of the hydrogen and nitrogen mixture in circulation, by water or by cooling.

The oxidation of this ammonia yields nitric acid, a substance that is indispensable for the manufacture of modern high explosives. The importance attached by Germany to this process is indicated by the fact that the Haber process was made a State monopoly. It is impossible to say how many tons of liquid air were made in Germany during the war, but we know that a single liquid air unit of plant was producing nearly as much as the total daily production in this country.

SOME OF THE CHARACTERISTIC PROPERTIES OF LIQUID OXYGEN.

These were demonstrated as follows :—

1. Absorption Spectrum : As would be expected from the greenish blue tint of the liquid, definite absorption bands are present in the spectrum. A clear cylindrical vacuum vessel containing liquid oxygen was placed in front of a slit, through which an arc lamp cast a parallel beam of light. When this was projected through a direct vision prism on to the screen three well-defined dark bands were seen in the orange and green in the neighbourhood of the C.D. and F. lines.*

2. The magnetic properties were shown by the action of a magnetic field on the liquid as it fell from a special silvered vacuum vessel † between the poles of an electro-magnet. By rotating a sector wheel in the illuminating beam the drops remained separate while no current passed round the coils of the magnet, but the excitation of the field at once caused the drops to coalesce into a

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* Proc. Roy. Soc., xlvi. 222.

† Proc. Roy. Inst., xx. 586.

thin stream. When this was moved slowly across the gap it was at first bent out of the vertical, and finally was all attracted to one pole of the magnet, to which it adhered in a bubbling mass.*

3. The spheroidal state of the liquid was shown by the mobility of drops of liquid air falling on the surface of some tetrachloride of carbon. These darted about, enveloped in their cushions of vapour—comet-like clouds of condensed tetrachloride. This was in striking contrast to their behaviour when scattered on sulphuric acid, on which they scarcely moved, but left small frozen areas floating about as the drops rapidly evaporated. Another example was provided by a faintly glowing piece of charcoal thrown on the surface of some liquid oxygen. The slow combustion of the charcoal at once increased to brilliant incandescence as it rushed about on the continuously renewed cushion of oxygen gas.

4. The application of liquid oxygen for explosive purposes was illustrated by soaking a wad of cotton in liquid air and applying a light to it. It instantly burnt with a brilliant flash like guncotton. Finely divided organic matter mixed with liquid oxygen can be used for blasting, when packed into a rock boring, and detonated by an electric discharge. It has the advantage of retaining its explosive properties only for a short time, with consequent absence of danger from any unexploded charge.

5. The high vacuum produced by charcoal in liquid air was exemplified in the mercury-vapour-radiometer first shown in 1907.† The radiometer was provided with a drop of mercury in a side tube and with a gram of charcoal in the bulb below. With the charcoal and mercury both cooled in liquid air, no activity could be induced, even with a condensed arc beam focussed on the vanes, showing that the pressure had been reduced to below $\frac{1}{50}$th of a millionth of an atmosphere. When the vapour pressure of the mercury was increased to this value by allowing its temperature to rise to -20 C., the vanes started rotating.

APPLICATION FOR PURPOSES OF RESPIRATION.

An important war-time application of liquid oxygen was to supply oxygen gas for airmen at high altitudes and also for treatment of "gassed" cases in trench or hospital. For this purpose the liquid was volatilised from some form of vacuum container by regulated influx of heat, whence the gas was conveyed by tubes to mouth-pieces for breathing. Apparatus of this kind was found in German aeroplanes brought down when raiding London. This method has the advantage over that of carrying the necessary oxygen compressed in steel cylinders, in that it is much lighter to carry, and less cumbersome.

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* Proc. Roy. Inst., xiii. 698.

† Proc. Roy. Inst., xviii. 755.

Three methods were devised for this purpose. They were in principle as follows :—

1. Syphon Evaporation.—A vacuum container of liquid oxygen was provided with an air-tight screw stopper ; a release valve in the stopper maintained a constant internal pressure produced by the slow evaporation of the liquid oxygen. A delivery tube passed from near the bottom of the liquid in the vacuum container through the neck to an external metal chamber, the evaporator. This had an outlet tube carrying a regulating valve. By opening this valve the pressure fell slightly in the evaporator and allowed some liquid to flow out from the vacuum container into the evaporator, where it was rapidly evaporised, the rate of flow and consequent evaporation being thus under perfect control.

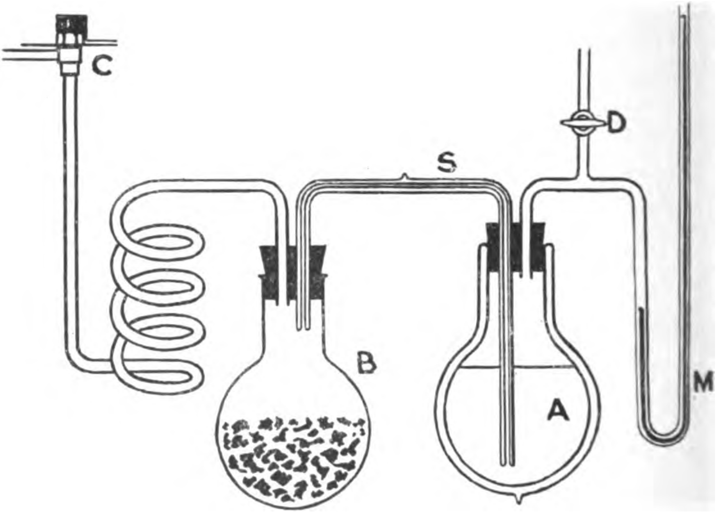


Fig. 1.

2. Electrical Heat Evaporation.—A small heating coil was secured on the bottom of the vacuum container, and thin leads passed from the coil through an air-tight insulated connection in the neck. All that was necessary to control the rate of evaporation was a small rheostat in circuit with the heating coil, and a supply battery. The oxygen was delivered through a tube in the screwed stopper. A release valve is only needed at reduced pressures, as, e.g., in the upper air, when it is placed on the delivery tube.

3. Metallic Heat Conduction.—A bunch of metal rods was secured in the centre of the flask by a non-conducting tube fixed under the screw cap. To start the evaporation a solid rod of copper or aluminium was inserted through the stopper and down the non-conducting tube to make a regulated contact with the immersed metal rods. This varied the influx of heat and consequent rate of evaporation. The oxygen was delivered as in method 2.

In all cases sufficient length of metal tube was added to warm the gas up to ordinary temperature before issuing at the nozzle for respiration.

Method 1 is illustrated in Fig. 1.

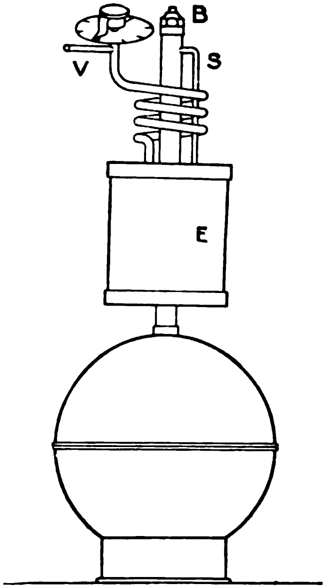


Fig. 2.

A is a glass liquid oxygen container provided with a mercury manometer M and release cock D. A glass vacuum syphon S connects A to the evaporating chamber B, which is a flask half filled with copper turnings. The outlet from B is a coiled copper tube terminating in a screw release valve C. When the slowly evaporating oxygen in A had increased the pressure to over half an atmosphere, as registered by the manometer, D was opened sufficiently to prevent any further increase of pressure. On opening C, liquid oxygen oscillated between A to B keeping up the necessary supply, the gas passing out through the coiled copper tube, which warms it to ordinary temperature before issuing at C. One practical form of such an evaporator constructed of metal is shown in Fig. 2. This is a spherical vacuum container made of copper, nickel or brass. The delivery tube S (corresponding to the vacuum syphon S of Fig. 1) is shown issuing from the neck under the release valve-stopper B (acting in the same way as D in Fig. 1), and passes down to the annular evaporating chamber E. The outlet from this consists of a spiral coil of copper pipe wound round the neck and terminating at the regulating valve V. Different forms of containers are represented in Fig. 3.

What may be called an emergency form was adapted from an ordinary glass thermos flask. This was suitable for easy handling in hospital or trench, and is illustrated in Fig. 4(*a*). The neck of the inner glass flask A is fitted air-tight into the outer metal case by means of rubber washers. The lip of the screwed cap C was also provided with a rubber washer. The delivery tube S was soldered through the top, where also the combined screw stopper and blow-off valve B was fixed. The evaporating chamber E surrounded the lower portion of the outer case, and from this the spiral outlet tube was wound round the vessel above E, up to and ending in the regulating valve V. The delivery tube S was bent at right angles outside the cap and led down to E by a union N, and a short flexible tube. This allowed the requisite play for tightly screwing on the cap. Such a vessel provided a steady supply of oxygen of about two litres a minute, sufficient for one person for respiration for over five hours. It weighed less than three kilos. when full and complete in a wicker basket, in which it was fixed for protection.

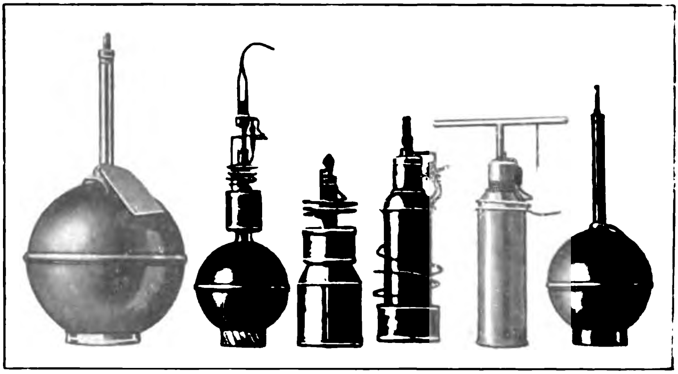
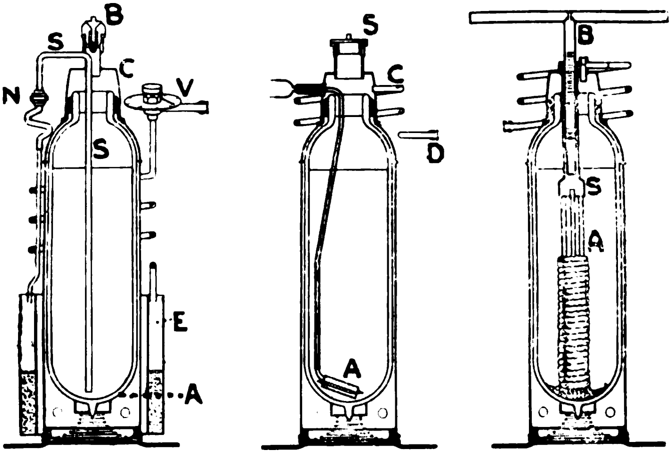


Fig. 3.

In many forms the cap C was made into a vacuum jacket stopper, provided as usual with a little charcoal for making the vacuum more effective when the cap became cooled. The object of the vacuum stopper is to guard against too rapid a flux of heat, and consequent rise of pressure by splashing or inclination of the contained oxygen in the glass vacuum vessel.

The vessels are liable to excessive loss by too rapid evaporation through the blow-off valve. This is likely to occur when the bore of the outflow syphon tube is too small. The liquid is not freely delivered into the evaporator, where there is ample space and surface for smooth boiling, but is partly volatilised in the narrow outlet tube. Consequently back surges of pressure into the container occur with accompanying rushes of evaporated oxygen, lost through the valve. This is most marked when the vessel is full and the free space therefore small. When a vacuum syphon delivery tube was fitted this did not occur.



(a) (b) (c)

FIG. 4.

With the evaporating chamber in the form of a spiral of copper tube (equivalent to restricting the free space without reducing the heating surface), the irregularities on continuous working were greatly increased, and the loss through the blow-off sometimes actually exceeded the rate of delivery. With a larger metal apparatus consisting of a 3-litre nickel vacuum container whose evaporator was in the form of three cylindrical brass vessels (capacity 200 cubic centimetres each), connected by coils of ¼-inch copper tubing, the loss through the release valve was between 55 and 65 grams per hour, while the delivery was maintained at 425 grams per hour (nearly six litres a minute, sufficient for three persons). Thus the escape through the blow-off amounted to about 15 per cent. of the total delivery for respiration, or 75 to 80 per cent. of the normal loss of the vessel when full of liquid oxygen with the delivery valve closed, and the release valve set to blow off at a plus pressure of half an atmosphere. It is therefore an advantage to lead the oxygen escaping from the release valve into the breathing tube circuit.

ELECTRICAL EVAPORATION.

A commercial vacuum flask adapted for electrical evaporation is shown in Fig. 4(*b*). The heating coil A was wound on a cross mica frame. Two asbestos-covered copper leads were sufficiently rigid to keep the coil in position near the bottom of the flask. The two leads passed tightly through holes in a short length of fibre rod B screwed into the metal cap of the flask. A little melted bitumen mixture secured an air-tight insulated joint. A short spiral of copper tube was fixed into the opposite side C of the metal cap to the fibre rod. The blow-off valve was at the end D of this delivery tube when necessary. The vessel was filled through a wide flat shouldered screw stopper S. The dimensions of the heating coil depended on the voltage of the electrical supply available for use with the flask.

An exceptionally regular flow of oxygen gas is always obtained from this arrangement, and the rate is only slightly affected by external conditions of temperature, etc. It has also the advantage that apart from the small heating coil no fittings or alterations are necessary to the actual vacuum container, as the simple electrical equipment is quite separate, and there is little in the container itself to get out of order.

METAL CONTACT EVAPORATION.

The fittings necessary to adapt a similar flask for heating by metallic conduction from the outside are shown in Fig. 4(*c*). A bundle of seven equally spaced aluminium rods A, ¼ in. diameter and 6 in. long, were screwed into a copper plug S. This was provided above with a slightly coned hollow socket to fit the end of a T-shaped aluminium rod B screwed down through the cap. The copper socket plug was secured air-tight to the screw cap of the vacuum flask by a german silver tube, easily clearing the inserted aluminium rod B. A copper spiral delivery tube was fixed horizontally into an elbow in the top of the screw cap while the vertical arm of the elbow held the small screw plug through which the flask could be replenished.

In this form of the apparatus the rate of evaporation depends largely on the surface of contact between the metal rods and the liquid oxygen in which they are immersed. As the liquid is used up the depth of immersion decreases; when it passes a certain point, defined by the dimensions of the rods, the rate of evaporation falls off. To prevent this it was found sufficient to wind absorbent asbestos rope round the bunch of conductors, leaving a teased-out brush spreading over the bottom of the flask. In this way the liquid was sufficiently distributed over the metal surface to keep the evaporation uniform, down almost to the last drop. The surface of the external conductor must also be sufficiently large to collect the necessary heat from the air; and the cross section of the neck of the conductor sufficient to convey this heat down into the liquid. The rate of evaporation will therefore vary somewhat with the external temperature. It was found that a commercial vacuum flask of a capacity of one quart could readily carry the necessary metal conductors to provide an evaporation of two litres per minute for over four hours. The variation was not more than 10 per cent. With a rate of three litres a minute, easily maintained for two hours, the variation was only 2 to 3 per cent. If the conductors were allowed to accumulate a thick coat of condensed ice and snow, the rate diminished by as much as 20 per cent. It was necessary to guard the neck socket also from ice obstruction.

The effect of variations of external temperature is seen with a vessel of water fitted on the conductor. When the temperature of the water fell from 17° C. to 7° C. in thirty minutes, the rate of delivery fell from 2·55 to 2·3 litres a minute.

SAFETY IN CARRYING.

In order to protect the vessels from rough handling various devices were tried, taking the general form of a double cylindrical case, with shock-absorbing arrangements between. A simple and very effective form was a plain cylindrical basket (shown partly cut open, Fig. 5), large enough to leave a space of nearly 2 inches all around the apparatus, which it carried upon a separate wicker disc A; this was supported by four indiarubber cables B about 1 centimetre in diameter, secured under the rim of the basket; four similar shorter fastenings C secured the disc to the bottom of the outer case. The cap of the vacuum flask D was kept central by three equally spaced loops of similar rubber cable secured at equal distances round the basket carrier at the level of the screw cap (one of these is indicated by E, Fig. 5).

A fully charged vessel in working order would usually survive the shock of a fall of about three or four feet when so supported: steel or whale-bone springs in place of the rubber cables were found to be too rigid in the restricted space available.

When using the vessels for any form of oxygen supply it was found safer to have some holes near the top and bottom of the outer metal case. The glass neck of the vacuum flask was also covered in all cases, to the depth of an inch inside and outside, with thin assay lead (see Fig. 4) to minimise the risk of fracture through splashes of liquid air when the vessel was filled, or from accidental contact of the inside metal parts, when fixing the cap. It is also an advantage to soak any corrugated paper packing in melted paraffin, to prevent the absorption of water when cooled, and to facilitate the insertion of glass refills. The glass refill must fit air-tight into the neck of the outer metal case.

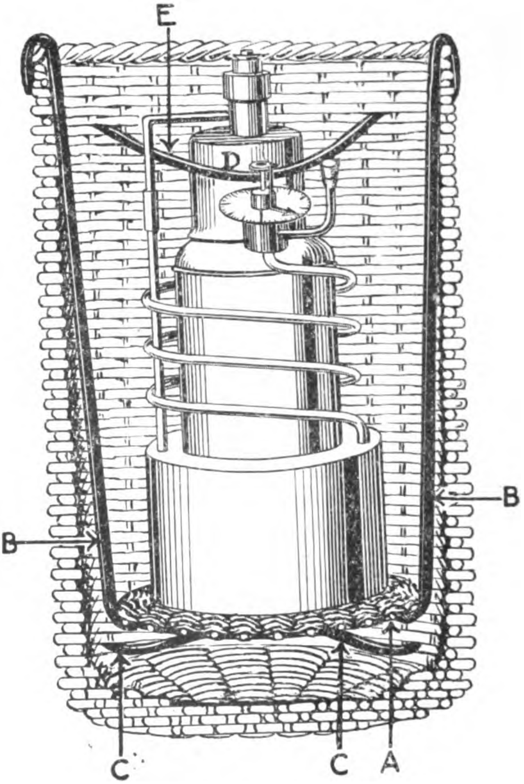


FIG. 5.

OXYGEN SUPPLY IN UPPER AIR.
EVAPORATION AT REDUCED PRESSURES.

When respirators are employed in aerial navigation they are subjected to changing conditions of temperature and pressure with varying altitude. When ascending the lowering of external pressure (Fig. 6) causes a considerable increase in the normal rate of evaporation of the liquid oxygen in the container. This is shown by the curve (Fig. 7) comparing the normal loss with the additional evaporation caused by lowering of external pressure. When descending the process is reversed, so that not only evaporation may cease, but recondensation may take place should the release valve leak at all. The temperature alterations which occur at the

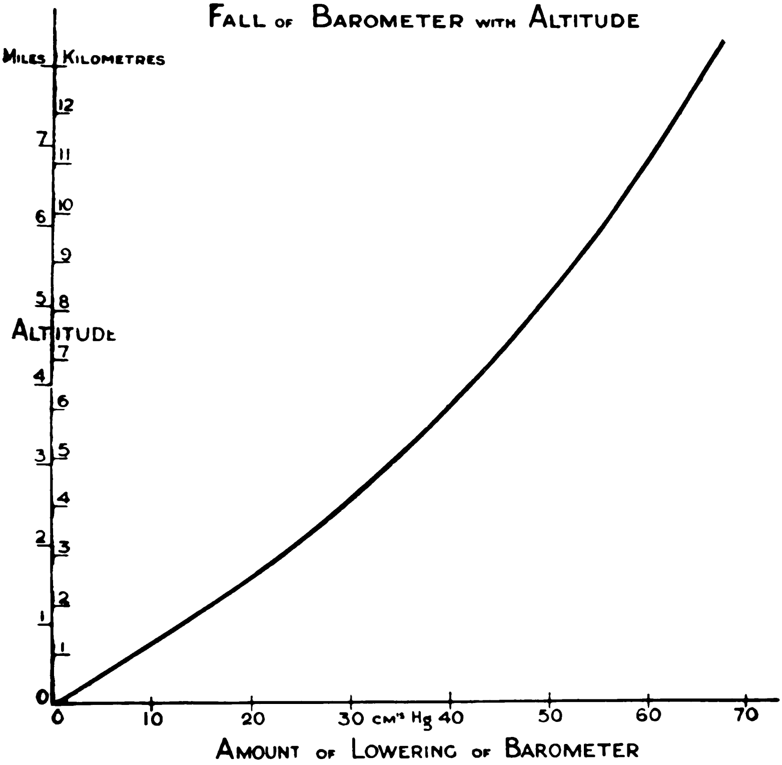


Fig. 6.

same time counteract in some measure the effects caused by altering pressure. The response of the evaporators to such conditions was examined by the arrangement shown in Fig. 8. The delivery valve V of a container A fitted for electrical evaporation was connected to a small vacuum pump B capable of maintaining a pressure of a few millimetres of mercury with a very rapid evaporation. The delivery was measured on a gas meter (not shown) connected to the pump outlet. The lowering of pressure was measured on a mercury barometer column C between the pump and the evaporator. A

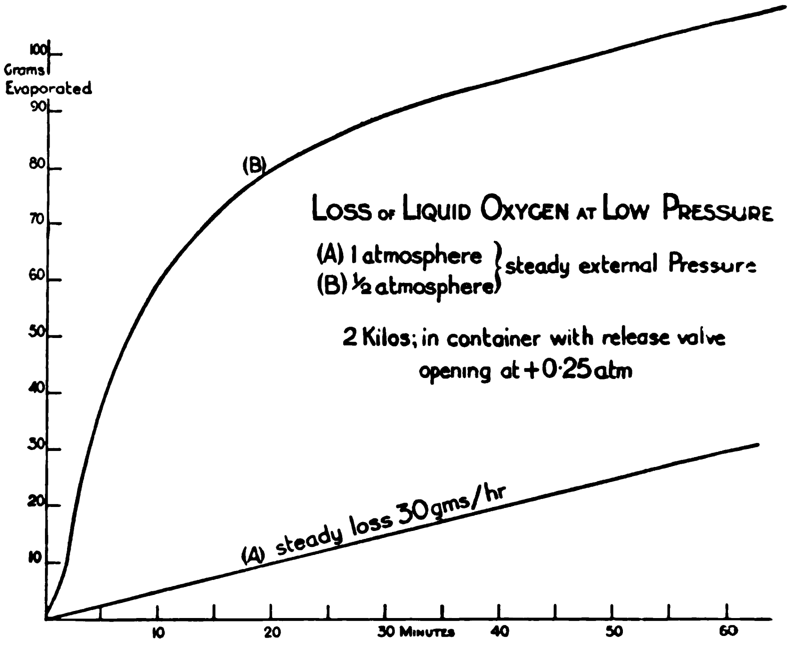


Fig. 7.

10–12 litre flask D was included in the circuit as an equaliser, while the three-way stop cock E served to regulate and stabilise the pressure as required. The rate of delivery at successive reductions of the heights of the barometer was thereby quickly measured, as well as the response of the machine to rapid alterations of height, up and down. A manometer was connected to the container to show the pressure at which the liquid was boiling. The indications of both manometer and barometer are shown in the curves (Fig. 9). Before applying the exhaust, the blow-off pressure at the release valve was steady at 18 centimetres of mercury above the atmosphere.

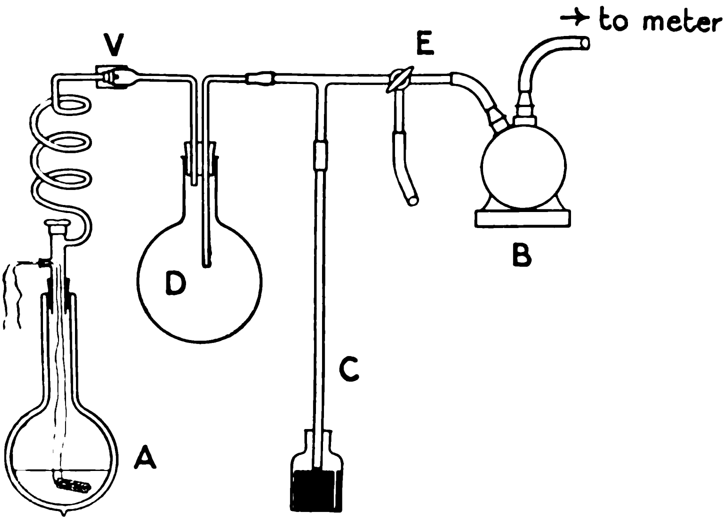

V
E
→ to meter
D
B
C
A

Fig. 8.

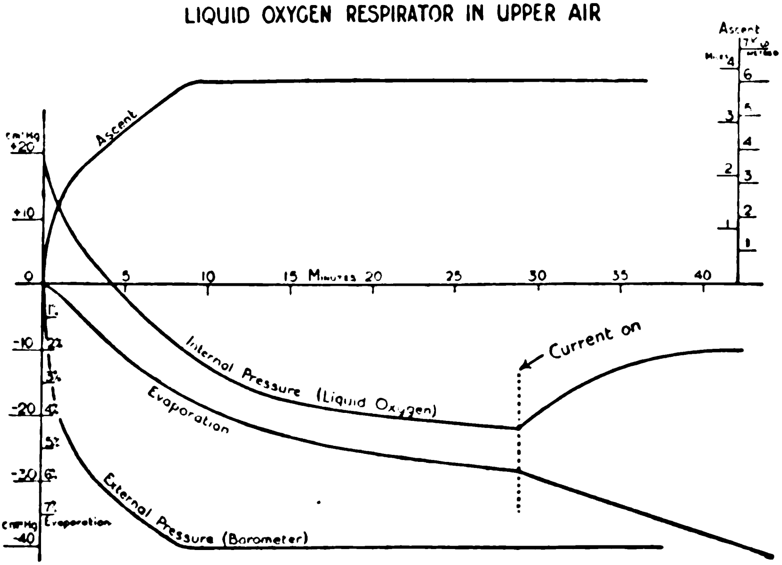

LIQUID OXYGEN RESPIRATOR IN UPPER AIR
Ascent
cm Hg
+20
+10
0
5
10
15 Minutes 20
25
30
35
40
Ascent
Miles
4
3
2
1
7
6
5
4
3
2
1
-10
-20
-30
cm Hg
-40
1%
2%
3%
4%
5%
6%
7%
Evaporation
Internal Pressure (Liquid Oxygen)
Evaporation
External Pressure (Barometer)
Current on

Fig. 9.

As the exhaust increased, this internal pressure fell, but not at the same rate. The difference of rate of fall of the manometer and barometer gives the lag caused by the cooling of the liquid oxygen. With a steady reduction of pressure of 34 centimetres of mercury (corresponding to an ascent of 3 miles) the internal manometer fell 28 centimetres in 23 minutes, and then required a further 20 minutes to reach the full reduction of 34 centimetres. The curve (Fig. 9) shows a more rapid reduction to 40 centimetres below normal (4 miles ascent) in 9 minutes. A lowering of 28 centimetres was then registered by the liquid oxygen, and the remaining 12-centimetre fall took a further 20 minutes, while the outside barometer was kept steady at the 4 miles level. When this point had been reached, nearly 6 per cent. of the original liquid oxygen had been evaporated, and its boiling temperature was reduced from 90° Absolute to below 83° Absolute.

To supply sufficient oxygen for two persons (4 litres a minute measured under ordinary conditions) a steady current of 16 watts was passed through the coil in the liquid oxygen. The internal manometer rose in the manner shown by the curve (Fig. 9) and the evaporation increased, the external pressure being maintained constant. In 5 minutes a delivery of 2 litres a minute was reached, 3 litres a minute in 9 minutes, and the full rate of 4 litres per minute in 18 minutes, by which time the inside pressure had increased 12 centimetres. A more rapid increase in the rate of delivery was easily obtained by a larger initial current, which was reduced as the rate increased.

EVAPORATION FROM CONTAINERS NOT IN VERTICAL POSITION.

The use of vacuum containers in circumstances where they were liable to movement about the vertical position would result in the liquid being thrown and splashed about in the vessel; but by packing the interior with a suitable liquid oxygen absorbent this does not occur. The best substance for the purpose was determined as follows:—

Equal bulks of various materials were packed into a silvered glass vacuum vessel. They were then saturated with liquid oxygen, the superfluous liquid drained off, and the amount retained weighed. The following table gives the results in grams; the volume of space filled with each absorbent being 350 cubic centimetres.

	Weight of liquid oxygen held.
1. Cocoanut charcoal (coarse)	50 grams
2. Cocoanut charcoal (fine)	185 ,,
3. Respirator charcoal	100 ,,
4. Lightwood charcoal	140 ,,
5. Asbestos	290 ,,
6. Sponge	80 ,,
7. Kieselguhr	225 ,,

These results showed that asbestos and silica dust (kieselguhr) absorbed the largest amounts of liquid. Relatively to the liquid oxygen capacity of the same space without absorbent, asbestos held 80 per cent. and kieselguhr 70 per cent., while the figure for the best form of charcoal (dust) was only 53 per cent. Moreover, silica suffered from the disadvantage of weight: the proportion absorbed on this basis being:—

Silica	Equal weight.
Asbestos	Three times its weight.

Thus the weights of the same vessel filled respectively with the two substances and saturated with liquid oxygen are, in grams:—

		Solid	Liquid	Total Weight
	Silica	225	225	450
	Asbestos	97	290	387
But the really effective basis of comparison is that of equal weights of liquid; in this case the proportions are:—	Silica	290	290	580
	Asbestos	97	290	387

so that the charged asbestos has only two-thirds of the weight of kieselguhr carrying the same weight of liquid.

Such masses, when in a vacuum vessel, retain a constant temperature and rate of ebullition until almost the last trace of liquid has boiled away. This was determined by immersing the gas bulb of a small helium thermometer, carrying a capillary mercury manometer,* into the centre of a cooled mass of silica contained in a silvered vacuum flask. In contrast to this, a similarly saturated mass of powdered charcoal shows a gradual rise of temperature with a steadily decreasing rate of evaporation. This follows from the exceptional gas absorption of cooled charcoal. The method has been applied to the study of this subject, among others connected with low-temperature radiation.

For the purpose of either the electrical heating or metallic conduction methods of evaporation already described, there is no objection to the use of a proper absorbent for the liquid oxygen, since the heating coil or metallic rods are readily embedded therein; but for the syphon method a modified arrangement is necessary.

The method adopted was to use a pure lead tube, which is flexible in liquid air or oxygen, weighted at the lowest part to keep it below the surface of the liquid. A double conical spiral of lead tube gave good results when connected to the rigid delivery tube cut-off just above the centre of the container. A brass sphere was pierced and fixed on the lower end of the spiral lead tube, its weight being supported by a light brass chain secured at the joint to the fixed delivery tube. With a pure lead tube of 3 mm. diameter and 1½ mm. bore the weight of the sphere was about 135 grams. The two spirals were both wound about the axis formed by the brass chain. The

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* Proc. Roy. Inst., xx. 583.

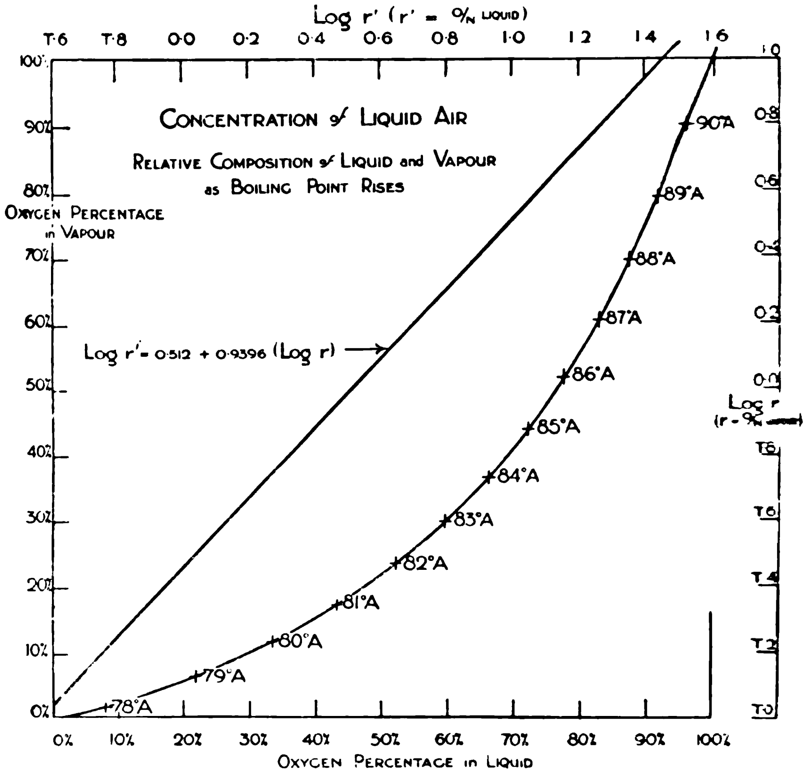

Fig. 10.

inner spiral was carried down only three or four turns, then brought up to form the outer spiral of nine or ten turns, which decreased in diameter down to the sphere.

When the rigid vertical delivery tube was deflected 45° the resulting displacement of the sphere at the bottom of the spiral was nearly 10°; on returning to the vertical the sphere went a few degrees beyond. After several movements the final displacement of the sphere was practically nil, the maximum distortion during tilting being about 15°.

LIQUID AIR COMPOSITION.

When only liquid air instead of liquid oxygen is available, the respirable quality of the product must be considered.

Liquid air as usually obtained varies in composition and boiling point, roughly, between 34 per cent. oxygen and 66 per cent. nitrogen boiling at 80° Absolute, to 100 per cent. oxygen (no nitrogen) boiling at 90° Absolute.

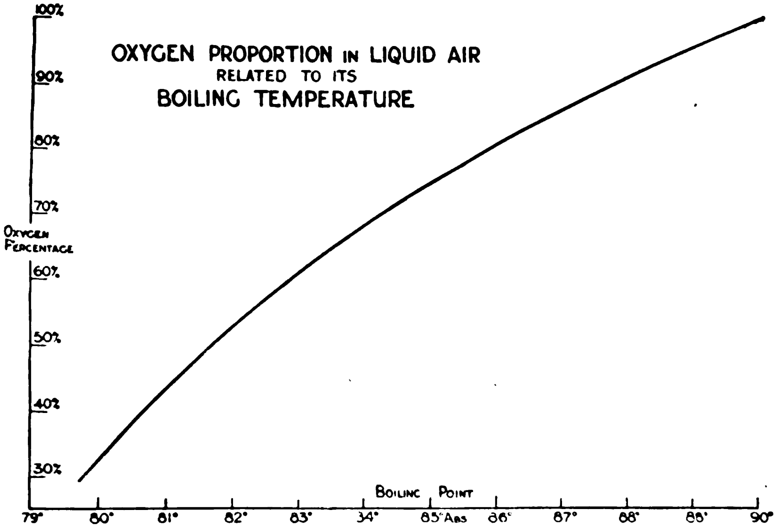

Fig. 11.

The corresponding composition of the vapour rising from it will be 12 per cent. oxygen and 88 per cent. nitrogen from the liquid boiling at 80° Absolute, increasing up to the final pure oxygen from the liquid at 90° Absolute. This is shown in Fig. 10, in which also is included the logarithmic relation between the proportion of each constituent in the liquid and vapour as given by Baly,*

$$\log. r' = 0{\cdot}512 + 0{\cdot}940 \log r,$$

when r and r' are ratios of oxygen to nitrogen in the vapour and liquid respectively. Fig. 11 is another curve applicable to the practical handling of liquid air, in which is shown the relation of the boiling point to liquid composition ; while Fig. 12 shows a similar relation of boiling point to the composition of the vapour : (the varying density of the vapour is also shown).

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* Travers' Gases, 1901, 224.

The respirable quality of the evaporated air will therefore vary according as the liquid is evaporated by conduction of heat into the whole quantity of liquid in the vacuum container, as in methods 2 and 3, or is totally volatilised in successive small quantities, as in the first (syphon) method. In the former case there will be some fractionisation of the more volatile nitrogen in excess of the less volatile oxygen, with the result that the vapour is less rich in oxygen than the liquid ; whereas in the syphon method the liquid is successively evaporated as a whole, and therefore gives continuously a

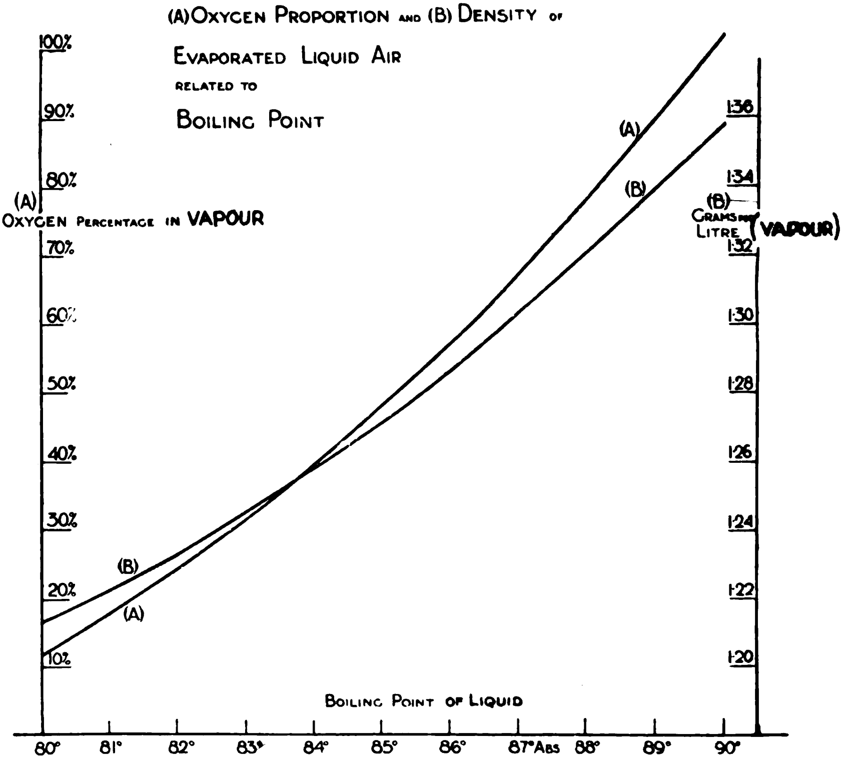

Fig. 12.

vapour of almost identical composition. This method is therefore of more advantage when almost new liquid air has to be employed without much concentration of the oxygen.

The rate of loss of liquid air is best determined by periodical weighings of the containers, but a rapid estimate can be made by connecting them to a $\frac{1}{20}$th cubic foot gas meter (or its equivalent) to measure the rate of evaporation. This meter can be appropriately scaled so as to indicate directly the weight loss. The factor employed is deduced from the curves just given, and varies by ± 5 per cent. with the composition of the liquid (as indicated by the boiling point). Fig. 13 shows how the loss per hour or per day, as required, is directly deduced from the measure of the volume evaporated in a stated number of seconds, and how this period varies with the temperature of the liquid air.

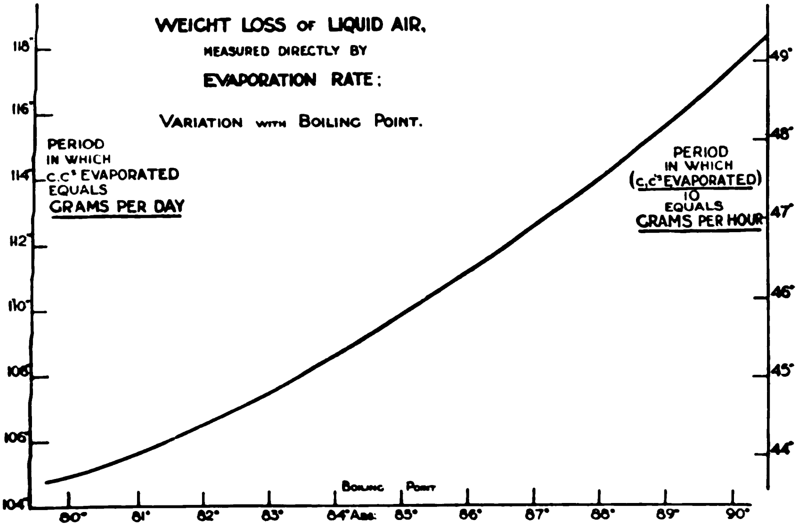

FIG. 13.

METAL STORAGE VESSELS.

The handling of liquid air and oxygen, apart from laboratory use, is only practicable with metal vacuum containers; and it is the absorptive power of cooled charcoal which enables the necessary high vacuum to be maintained, and thus makes it possible to construct such vessels. The first vessels to give satisfaction were described in

1906.* The requisite heat isolation necessary to raise the standard of metallic vessels to anything like the same efficiency as those made of silvered glass, can only be attained by careful construction. The three main drawbacks to the use of metal instead of silvered glass are: (1) increased conductivity; (2) decreased reflecting power; (3) greater difficulty in securing and maintaining the necessary very high vacuum.

1. The first objection is met by reducing the diameter and thickness of the double neck, as this is the only bridge between the inner and outer shells by which conduction of heat can take place. But while the diameter and thickness are made as small as possible, the neck is made as long as is consistent with rigidity and handiness. By so doing not only is the conductivity loss avoided, but the convection disturbance caused by the high temperature gradient is greatly reduced. The proportionate losses due to all these neck effects became rapidly less as the capacity of the flasks was increased.

2. No metallic container can be really efficient unless the surfaces exposed in the annular vacuous space are highly polished. The final buffing must not involve the use of any oily material, as this would increase the emissivity, and add to the difficulties of the subsequent exhausting.

A copper surface is preferable to one of brass; and several successful copper containers up to 12 litres capacity were constructed. In these cases, however, the neck was made of stiffer and less conducting alloy. With capacities greater than 12 litres a more rigid metal was also desirable for the spherical shells, to withstand distortion. Gilding-metal brass of good quality is preferable to copper, despite the greater emissivity of the former; but with all large vessels it is necessary to strengthen the equatorial joint, more especially of the inner shell. The soldered joint must have quite 2 cm. depth of close bearing surface all round. An additional precaution is afforded by brazing not less than six circular flat lugs round the rims of each of the separate halves of the shell. The lugs are on the inside of the upper half, which fits into the lower half with the corresponding lugs on the outside. Before soldering the joint between the two, the inner and outer lugs are screwed together all round, the necessary holes having been previously bored and tapped. This construction was especially desirable in the larger copper vessels.

EXHAUST OF METAL CONTAINER.

3. As already stated, the chief objections to metallic vessels are (*a*) the difficulty of eliminating minute pores and leaks, and (*b*) the difficulty of removing the gases absorbed both by the metal and the charcoal. The gases remaining in the pores of the charcoal are

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* Proc. Roy. Inst., xviii. 349.

only slowly evolved at the temperature that can be applied during exhaustion.* It therefore follows that the best results are obtained from a very high exhaust maintained for as long a period as is practicable; while keeping the vessel and contained charcoal as hot as is possible without softening the joints.

Owing to the difficulty in distinguishing between the slow evolution of gas from the pores of the charcoal, and a small leak in the vessel, it is necessary to test the construction for leak before the charcoal is finally introduced, by attaching to a vacuum circuit connected with a suitable manometer, from which readings are taken at intervals for several hours. After a good preliminary exhaust the charcoal is introduced and sealed up, the vessel is then ready for the final exhaust.

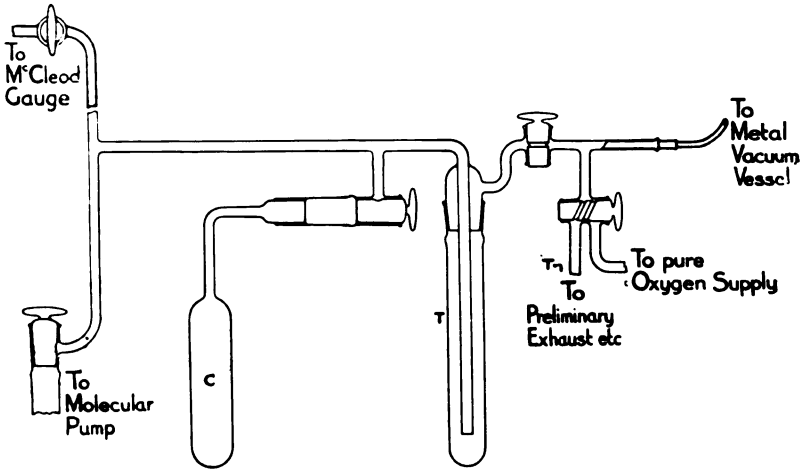

FIG. 14.

For the next stage a mechanical pump, capable of reaching an exhaust pressure of 0·00001 mm. Hg. is best; and for the final maintained exhaust, charcoal in liquid air is the simplest, the charcoal bulb having been heated and exhausted during the earlier stages by the mechanical pump. A good arrangement for this purpose is shown in the diagram Fig. 14. C is the charcoal container, preferably of quartz or metal, for rapid heating and cooling; and T the liquid air trap, to condense water vapour and other impurities from the metal vessels.

The progress of the exhaust of a 25-litre brass vessel containing

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* Proc. Roy. Soc., xviii. 439.

250 grams of cocoanut charcoal was as follows:—the operation consisted in alternating periods of exhaust and gas accumulation from the hot charcoal with the pump shut off. The vessel was supported (inverted) inside a lagged sheet iron dome for the purpose of being heated within and without by steam jets. The successive stages of the alternated exhausts and accumulated gas pressures were as follows:—

	Exhausts		Rests (Pump off)	
Time	Maintained Exhaust (mm. Hg.)	Stabilised Pressure (mm. Hg.)	Time	Pressure Increment (mm. Hg.)
15 min.	0·0008	0·045	20 min.	0·101
45 ,,	0·0002	0·0145	30 ,,	0·0465
35 ,,	0·00013	0·0061	30 ,,	0·0038
			{44 hours (cold)	0·0031
			{44½ ,, (hot)	0·0068
25 ,,	0·00010	0·0028	{35 min.	0·0152
			{65 ,,	0·0204
40 ,,	0·0030	0·0030	sealed off	

The steady diminution of gas accumulation is the best proof of a satisfactory vessel.

In order to seal off a metal vessel after exhausting, a pure lead tube is employed to connect it to the vacuum circuit. This tube is soldered at one end into a small brass bend brazed into the outer shell of the vessel, and at the other end into a straight light brass or iron tube, which slips into a slightly wider glass tube on the vacuum circuit. (See Fig. 14.) The metal to glass joint is made by sealing wax. The intermediate lead tube must be long enough to prevent this joint softening by the heating of the vessel during exhaust. When the exhaust is complete the lead tube is squashed together with a corrugated surface for about 4 or 5 cm. The corrugations are deepest in the centre and become less on either side. A clean cut is then made through the centre of the constricted portion and the cut end rapidly tinned. As an additional precaution melted bitumen or Chatterton compound is painted over the joint, and a metal cap secured over all.

The thorough cleaning of the charcoal is of the first importance if the best results are to be obtained. Very good results can be realised by a shorter treatment as follows:—Pure oxygen up to about 10 cm. pressure is admitted to the hot charcoal after, say, the second exhaust period. This is allowed to remain for about half-an-hour before again exhausting: after two or three repetitions the charcoal is rendered sufficiently active to maintain the vacuum required in vessels for ordinary purposes. The bad effect of poor charcoal was demonstrated in one case of an old container that had a relatively high rate of loss without showing any sign of leak. It was decided therefore to determine the state of the exhaust and what improvement, if any, could be made in the behaviour of the vessel. To attain this object a special vacuum-jacketed drill was fixed on its outer surface (shown diagrammatically in Fig. 15) for the purpose of

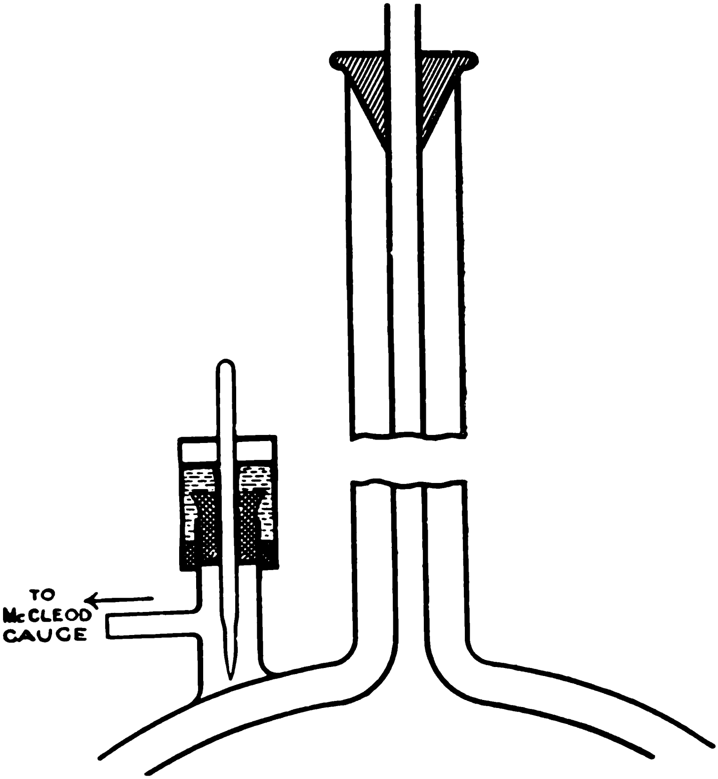

FIG. 15.—TESTING STORAGE VESSELS.

Piercing to Measure Exhaust.

opening the annular space to an exhausted McCleod gauge, to measure the pressure and determine how much the vacuum had deteriorated without altering the container in any way. The fitting employed consisted of a cylindrical flanged cap soldered to the outer surface of the flask to be examined. A side tube in this cap connected it to the vacuum circuit, while the neck of the fitting carried a small steel drill which could be rotated to and fro while protected by an oil-cup against influx of air. After exhausting and testing the arrangement for tightness, etc., connection was opened between the annular space of the container and the exhausted circuit by a small rotating of the drill. Subsequent examination revealed that a clean 2½ mm. hole was thus produced. The vacuum maintained by the flask was thus directly measured. The subsequent response of the flask to different degrees of vacuum was also measured. In the result it was ascertained that not only were the polished surfaces at fault, but also that the condition of the charcoal was improved by further heating and exhaustion, so that the rate of loss was materially reduced. Before piercing, the vessel had been losing 2·85 kilos. per day, whereas after the re-exhaust the charcoal was so much improved that the rate of loss was reduced to 1·64 kilos. per day. A pressure of 0·648 mm. was registered when the vessel was opened as described. There was no appreciable leak. With steam heating the pressure rose as follows, due mainly to gases expelled from the charcoal :—

Time	10 min.	30 min.	2 hr.	2½ hr.	3 hr.	3½ hr.
Press. in mm. Hg.	1·02	1·72	2·075	2·107	2·24	2·30

The subsequent exhaust was then carried out as previously described, with the following results :—

Exhausts		Pressure in mm. Hg.	Rests	Pressure in mm. Hg.
2nd	15 min.	to ·00017	30 min.	0·108
			16 hours	0·0206 at 15° C.
			16½ ,,	0·222 ,, 100° C.
5th	20 ,,	·00013	25 min.	0·023 ,, ,,
			(*a*) 16½ hours	0·0052 ,, 25° C.
			(*b*) 17 ,,	0·0357 ,, 100° C.
6th	30 ,,	·00010	25 min.	0·022 ,, ,,
7th	35 ,,	·00008	Steaming stopped	

On the vessel becoming cooled after the steam heating, liquid air was poured in. The pressure fell in a few hours to a limit of 0·000015 mm. Hg., and the rate of evaporation measured by the meter was one litre in 68½ seconds, equal to 1639 grams per day. It is interesting to note that from the Rankine equation, $\log. p. = A - \frac{B}{T}$ applied to the observations marked (*a*) and (*b*) above, between the temperatures 25° and 100° C, the deduced value of the B. (proportional to the latent heat of the occluded charcoal gases).

came out at 1120. The corresponding value for carbonic acid (taken at a much lower temperature) is 1350, while oxygen and nitrogen are of the order of 500. Hence practically all air gases had been removed at this stage, while carbonic acid alone was being evolved. This is a good indication of the clean state of the charcoal at that time, a conclusion which was supported by separate examinations of the gases evolved at successive stages under high exhaust from heated charcoal.

VERY ABSORPTIVE CHARCOAL, CALLED ACTIVATED CHARCOAL.

In the early days of the use of cooled charcoal for high vacuum work, it was soon recognised that good efficiency was only obtained when the charcoal was carefully prepared, free from volatile organic and inorganic constituents. In 1911 chemically prepared cocoanut charcoal (previously treated at a red heat with chlorine and then with hydrogen) was continuously heated in an electric furnace at 800° to 850° C. Periodical tests of the absorptive power proved that this quality rose steadily for two or more days. Values for different gases at atmospheric pressure are given in the following table :—

Gas Absorbed	Temperature of Charcoal	Vol. Absorbed per gram of Charcoal
Oxygen . . .	B.P. of Oxygen 90° Abs. . .	1680 cc.
Nitrogen . .	,, ,, Nitrogen 77° Abs. . .	1140 ,,
,, ...	at 86° Abs. (Liq. Air) .	700 ,,
Air . . .	,, ,, ,, ,, ,, .	883 ,,
Hydrogen . .	,, ,, ,, ,, ,, .	212 ,,
Carbonic Oxide .	,, 88° Abs. (Old Liq. Air)	733 ,,
Carbonic Anhydride	,, 195° Abs. (CO_2 snow)	676 ,,

Thus time is an important element in the preparation of such charcoal. A sample at the boiling-point of hydrogen (20·4° Abs.) absorbed 397 cc., the pressure on saturation being only 2·2 mm. Similar improvement takes place in ordinary unpurified cocoanut charcoal after prolonged heating.

EFFICIENCY OF VACUUM CONTAINERS: CONTRIBUTORY CAUSES. LOSSES DUE TO NECK APERTURE IN VACUUM VESSELS.

The proportionate loss due to the convection and temperature gradient in the neck aperture was estimated in several cases. Comparisons were made of vessels of similar dimensions but with

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* Proc. Roy. Inst., xviii. 752.

different sized necks. Stoppers of varying degrees of isolation and tightness were tried as to their effect on the rate of loss of the vessels to which they were fitted. The best arrangement was a plain hollow stopper, filled with the same liquid air as that in the vessel. This stopper was a cylindrical bulb sealed above to a tube held in an indiarubber bung fitting into the neck of the vacuum vessel. A small outlet hole was made in the bung by which connection was made to the meter for measuring the rate of evaporation of the liquid in the flask. Small silvered cylindrical vacuum vessels could sometimes be so employed (Fig. 16), but the stopper effect was not so complete; and further they took a long time to reach a steady reduction of rate.

A two-litre silvered glass vessel (referred to in curves 1 and 2 of Fig. 17) had a neck aperture of 37 mm. diameter by 70 mm. long. When this was closed by an easy fitting silvered vacuum vessel containing liquid air (Fig. 16) the rate of loss was lowered by 7½ per cent. With only one silver coating (on the inner shell), the saving effected by the same stopper amounted to 12 per cent. The corresponding reduction in metal vessels was much greater; thus a 25-litre vessel, with neck unprotected, lost 1·21 kilos. of liquid air per day (4·84 per cent. per day of the total contents). When the neck was kept at liquid air temperature by a wide tube full of liquid air fixed round it, the loss fell to 1·00 kilo. per day, or a reduction of 17·3 per cent. on the previous rate; the inner metal neck was 1·6 cm. diameter and 30 cm. long, while its walls were 0·2 cm. thick (much more than was necessary), 0·6 mm. being sufficient with suitable tubes. In a vessel of better construction the value of the reduction would therefore be relatively less than the value observed in this case.

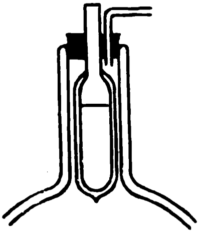

Fig. 16.

EFFECT OF EXTERNAL TEMPERATURE.

Appreciable alteration in the evaporation rate is also caused by changes in the room temperature, while temperatures such as are experienced in the upper air have a considerable effect. When a 10-litre copper container, whose neck tube was made of badly conducting alloy, was placed in an enclosure maintained at − 20° to − 25° C., its rate of loss was 23 grams per hour, as compared with 34 grams per hour in the room at 12° C.

RELATION OF VACUUM TO EFFICIENCY WITH DIFFERENT VESSELS.

The relation between rate of loss and degree of exhaust was studied with several vessels attached to the vacuum circuit (Fig. 14). The rate of evaporation of the flask was measured at different steady stages of the exhaust, down to the highest vacuum. This was easily controlled by the stop cock on the charcoal bulb, and measured on the McCleod gauge. Three of the resulting curves are shown in Fig. 17.

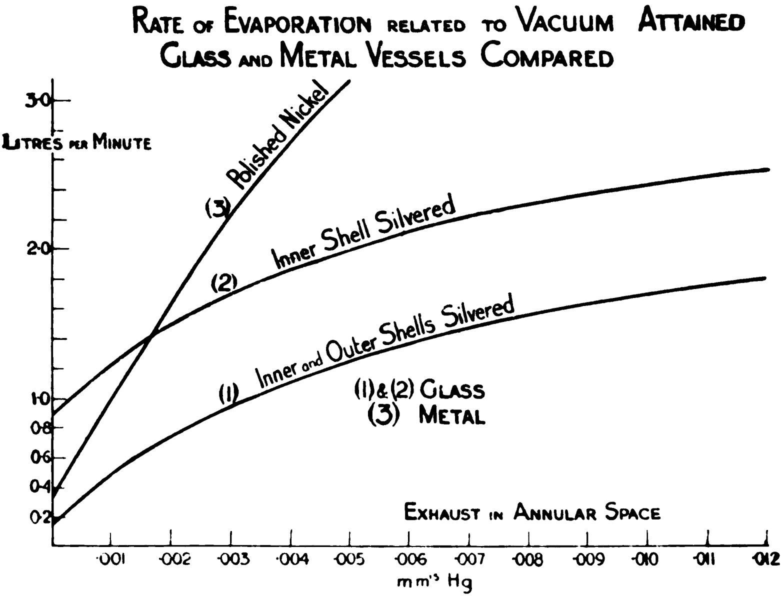

FIG. 17.

These curves serve to illustrate the rapid loss of efficiency of a metal flask as compared with one of glass, when the vacuum becomes deteriorated: they also show the relative values of the reflecting surfaces on the outer and inner shells respectively of a spherical silvered glass vessel. Curves (1) and (2) refer to a 2½-litre glass vessel, and (3) to a 3-litre nickel vessel. For curve (2) the silvering on the outer glass surface was removed. The efficiency of the vessel at the highest vacuum was thereby diminished to one-fifth—its rate of evaporation being increased five-fold; therefore approximately 80 per cent. of the total radiant heat seems to be reflected at the outer silvered surface. At the same time the rate of increase of evaporation with lowering of vacuum remained unaltered, as is shown by curves (1) and (2) being parallel. When the similar value for the nickel vessel is compared with this a much more rapid deterioration is found to occur, as appears from the steeper slope of curve (3). The actual increments were found to be an additional litre per minute per 0·001 mm. Hg increase in pressure in curves (1) and (2), and approximately double this value in (3). Above 0·001 mm. the increment is no longer linear, and when the pressure has risen in the glass flask to 0·010 (ten times the previous limit) the increment of evaporation is now one litre per minute per 0·0167 mm.; or about one-seventeenth of the initial value. These values should be considered in conjunction with the following table, comparing the efficiency of flasks with different reflecting surfaces. They were all exhausted to the limiting high charcoal vacuum, and filled to the same extent with old liquid air: the steady rate of evaporation was then observed. The total heat influx thus determined was reduced to calories per unit area per minute, and gave the following results:—

Vessel	Capacity	Material	Loss per Day		Calories per Unit Area per Min.
			Per cent. of Total	Grams	
1	6½ litres	Silvered glass	5·4	350	0·0078
2	1 litre	„ „	15·2	152	0·0116
3	25 litres	Brass	4·2	1040	0·0093
3A	25 „	„ inferior polish	6·56	1639	0·0138
4	12 „	Copper	6·25	750	0·0109
5	3 „	Nickel	17·6	528	0·0193

The "Emissivity" values in the last column are loaded by the neck losses, conduction and convection. These are relatively higher for smaller flasks: so that Nos. 2 and 5 bear disproportionately high apparent emissivities. No 2 was deduced from evaporation values given by Berry[*] for an ordinary pattern 1-litre silvered glass flask, thoroughly exhausted by cooled charcoal. The neck dimensions of such a flask are usually about 3 cms. diameter by 10 cms. long. The emissivity value obtained from a vessel of similar capacity but much narrower neck was even lower than for the 6½-litre vessel (No. 1 above), which however had the usual neck aperture of 3 to 4 cms.

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[*] Proc. Roy. Soc., A, lxxviii. 456.

These rough values are sufficient to indicate that the method is capable of yielding considerable information on emissivity problems at low temperature.

With metal containers of a uniform pattern, the relative efficiency in conserving their liquid air contents falls off very much as the capacity is reduced. This is seen by the comparison of the percentage

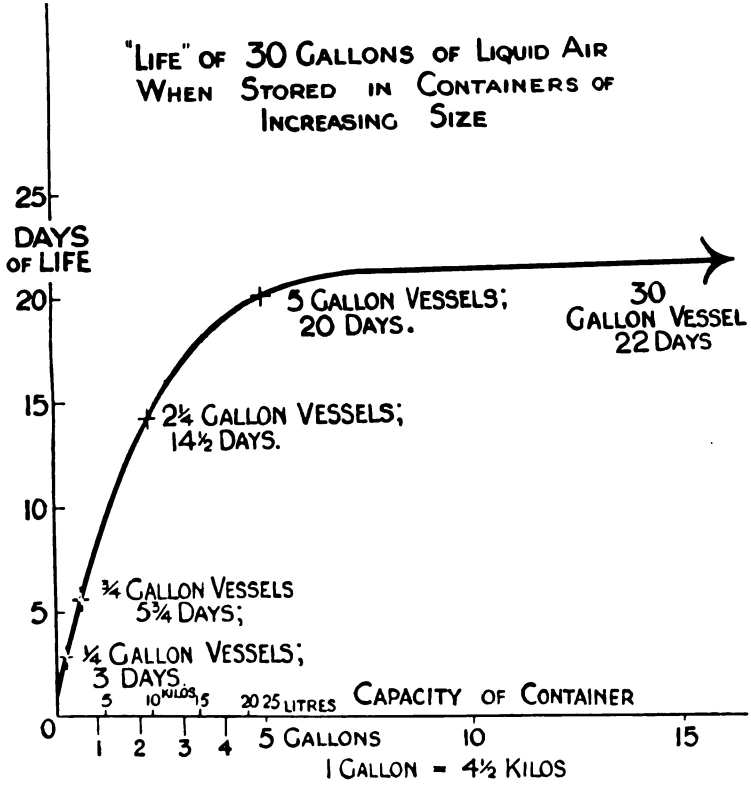

Fig. 18.

losses (4th column) for vessels 3, 4 and 5, and is further illustrated by the curve of Fig. 18, which shows the relation between the time taken for 30 gallons of liquid air to disappear by evaporation and the size of container employed to store such a quantity. The standard of efficiency assumed for this purpose is the low average of ordinary commercial vessels, and could be materially improved. From this curve it is clear that there is little advantage in increasing the capacity above 5 or 6 gallons (25 to 30 kilos) for storage purposes, at which size the limiting efficiency is reached of 4 to 5 per cent. loss of the total capacity per day. For flying purposes one gallon (5 litres) would be a useful mean size for supplying three or four persons, even though there might be one or two days' delay between the delivery of the liquid air and the flight in which it was to be employed.

May I remind the members that the expenditure on War work has resulted in a serious depletion of our funds? The Institution relies on the support of its members, and has had a hard struggle for existence. There is a popular fallacy that the Institution is highly endowed. (The Experimental Research Work during the past century has been executed at a total expenditure of £120,000, or at the average outlay of £1200 a year.) When the present Fullerian Professor of Chemistry was appointed in 1877, the Bequests and Legacies amounted to £3400. Thanks largely to the exertions of the Treasurer, Sir James Crichton-Browne, the Endowment has now risen to £70,000; but the annual number of new members elected has gone down to half the pre-war figure. The Royal Institution passed through a crucial period ten years after the Napoleonic Wars; it is hoped it will not have to face a similar crisis again.

[J. D.]

15 [illegible] 22

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